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The ADVANCEMENT OF SCIENCE

Vol. XII, No. 46

SCIENCE AND THE SCIENTIST

Presidential Address by

Sir ROBERT ROBINSON, O.M., F.R.S.

and

Addresses by

PRESIDENTS OF SECTIONS

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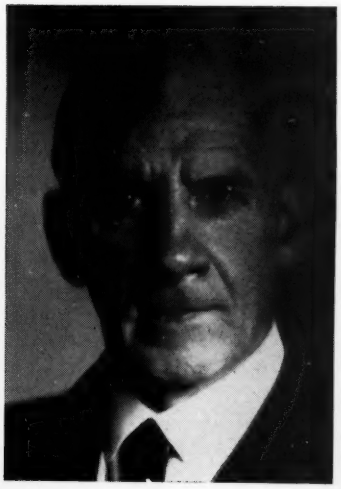

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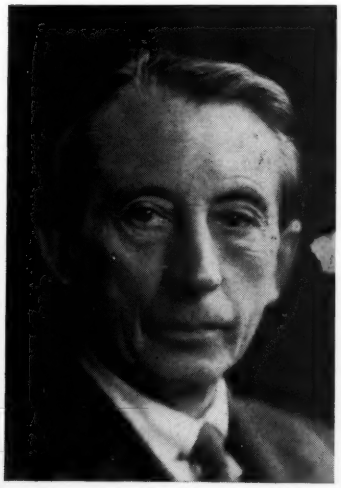

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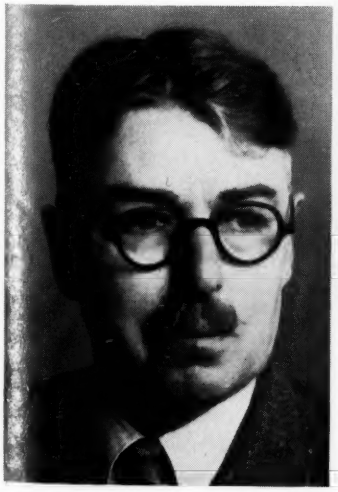

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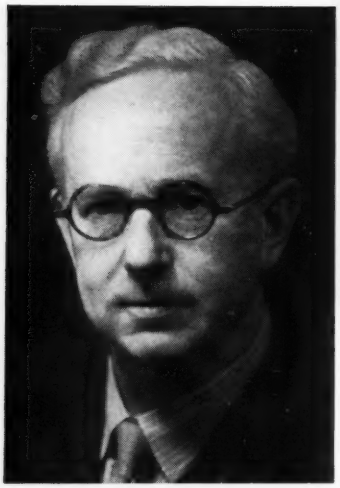

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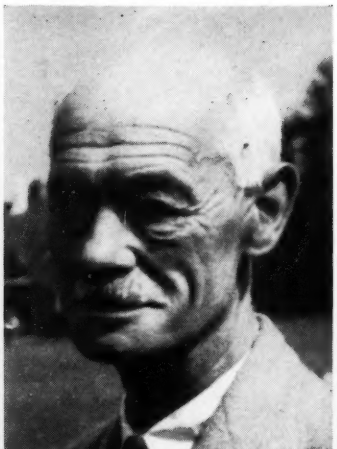

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SCIENCE AND THE SCIENTIST

Address by

SIR ROBERT ROBINSON, O.M., F.R.S.

PRESIDENT OF THE ASSOCIATION

THE Association held its sixth Annual Meeting in Bristol in August 1836 under the Presidency of the Marquis of Lansdowne. No significance need be attached to the fact that the well-known riots occurred here in the following year which is much better dated by the accession of Queen Victoria. I noted from the Report with interest and a certain wistful approval that the Address was delivered on that occasion not by the President but by Charles Daubeny, Fellow of Magdalen College, Oxford, Local Treasurer for Oxford. Daubeny had already been appointed to the University Chairs of Chemistry and of Botany. He was well qualified to profess Geology also, but his eventual third Chair was that of Rural Economy. He was a founder member of the Association and had attended the five previous meetings. Undoubtedly one of the chief pioneers and benefactors of science in Oxford he was a man of great originality and tireless energy. Among many notable contributions to knowledge, one that deserves the epithet classical, was his discovery that the process of photosynthesis occurred more rapidly under the influence of blue and violet light than under that of the less refrangible rays.

In the course of his Address, Daubeny referred, among other things, to various systems of notation in chemistry and mentioned in that connection, Dr. John Dalton, whom he described as the 'venerable' founder of the Atomic Theory but who was in fact just short of seventy years of age. At the meeting of the Association in the previous year at Dublin Dalton had given an account of his experiments on the distillation of caoutchouc and of the formation of a volatile oil which was doubtless crude isoprene. This he called 'double olefiant'. One volume of the vaporised substance exploded with six volumes of oxygen gave four volumes of carbonic acid gas which works out for :—

$$C_5H_8 + 6O_2 = 4CO_2 + 3H_2O + (CH_2O)$$

Here the CH_2O merely represents loss of partly oxidised hydrocarbon. I mention this matter, rather out of context, because the work seems to have been forgotten.

Dalton was not too 'venerable' either to be a Vice-President of the Chemistry Section at the Bristol Meeting or to be charged with the duty of examining the lesser constituents of the atmosphere. One wonders whether the small incombustible residue noted by Cavendish, and which was undoubtedly argon, was known to him.

The next meeting at Bristol was in 1875. The President was the eminent engineer, Sir John Hawkshaw, and he delivered a most interesting address on the historical development of achievement made possible by applications of engineering and other sciences. The last three sentences will interest you. '. . . Need we try to extend our vision into futurity further? Our present knowledge, compared to what is unknown, even in physics, is infinitesimal. We may never discover a new force—yet, who can tell?'

The meeting was attended by many famous men of science and among the Officers of Sectional Committees we find Cayley, Thomson (afterwards Lord Kelvin), Tyndall, Rayleigh, Williamson, Galton, Armstrong, and Tilden; a short list at random that could be greatly extended.

Sir William Crookes was the President at the third Bristol meeting in 1898 and he delivered an outstanding address on a variety of topics. First on the gloomy prospects for the supply of bread to the

people of these islands. Here he foresaw with characteristic intuitive genius that the only possible solution would be to obtain nitrogenous products from the atmosphere. At that time the only known method was the combustion of nitrogen with the help of a powerful electric discharge, an experiment initiated by Henry Cavendish, developed by Crookes himself in 1892, and by Rayleigh for the isolation of argon. Crookes even calculated that electricity generated from coal could be used to produce sodium nitrate at £26 per ton and that power from Niagara would reduce the cost to £5 per ton. The figure for Chilean nitrate was £7 10*s.* per ton, estimates which have only relative interest at the present time. If the Haber synthetic ammonia process had not offered a much better alternative, it is possible that Crookes's idea would have been commercially developed. In any case his thoughts were along the right lines.

Crookes was able to report that on May 10, 1897, Dewar had written to him: 'This evening I have succeeded in liquefying both hydrogen and helium.' Further, he mentioned that in the preceding few months, Ramsay and Travers (a distinguished Bristolian) had discovered three new rare gases of the atmosphere.

He goes on to speak of X-rays and the early discoveries of radioactivity by the Curies and Becquerel. Speculating on the source of the energy unceasingly radiated he found it in the translational motion of the molecules, and of the atoms within the molecules. He was thus led to the considerable underestimate that the air in a room of moderate size 'contains energy enough to propel a one-horse engine for more than twelve hours,' and continues, 'The store drawn upon naturally by uranium and other heavy atoms only awaits the touch of the magic wand of Science to enable the Twentieth Century to cast into the shade the marvels of the Nineteenth.' How much more true that is than Sir William Crookes imagined.

His final section on psychical research and experimental psychology was restrained but confident. He was conscious of the criticism which he was inviting but did not shrink from giving witness to his conviction that the phenomenon of telepathy, for example, was a real one and that the subject should be studied by scientists.

May I now interpolate that we should all agree with this. Telepathy, and even pre-cognition, are claimed to have been established and surely it is extremely important to know whether the evidence is really good enough to justify these claims. In the first place we need to discuss how to set to work, and only when that is quite clear to put a plan into operation. It is the first stage which is likely to be the more difficult. Perhaps it should be added that no final verdict can be anticipated. As our distinguished colleague Dr. Warren Weaver has so aptly remarked, there is no such thing as the voice of science.

The last Bristol meeting was in 1930 with Professor F. O. Bower as President. He spoke on *Size and Form in Plants* and remarked that this ninety-ninth year of the life of the Association marked the close of the Darwinian epoch. That was intended only in relation to personal links and did not imply that Darwin's influence on science had in any way lessened. Bower naturally referred to Sir William Crookes's remarkable address of 1898 and was able to point not only to the actual production of artificial fertilisers, but also to the development of new strains of wheat which enabled areas of lower rainfall to be utilised, and also shortened the period between sowing and reaping.

So far, in asking for your attention to the reverberations of the thunder of the past, I have been on solid ground but now the situation changes as I turn to my own choice of a topic. I recall the case of the Texas politician who was said, naturally by an opponent, to have 'both feet firmly planted in the air.' However, even if this is thought to apply, I hope to come to earth occasionally.

One of the troubles of the modern life of scientists is that we are all too busy and that only a small fraction of our available time and energy can be spent in practising our real vocation. Please forgive a personal reference—it is not really very boastful. I am conscious of a certain talent for tapping a test-tube but it is a long time since I was able to indulge this bent to much purpose. Lately I have had to be content to encourage others to perform this

simple operation with, it should be added, due consideration of what is placed in the test-tube, and observation of what happens.

When the Secretary, as agent of the Council, asked me for the title of this Address, I was more than usually preoccupied and selected 'Science and the Scientist' because I thought it clay that could be modelled into almost any desired shape.

However, there was at the back of my mind the feeling that the functions of the two are not always clearly understood and distinguished, so that there is more than a little confusion of thought in some quarters, especially in the use of the word 'Science'. The subject is not at all controversial but I will ask for your indulgence to state once more the generally accepted definition, clearly implied in common consent if not the subject of any recent manifesto.

Science is simply the record, enshrined in the literature, of the system of knowledge and of the conceptions correlating parts of that system.

It is fundamental that such knowledge cannot be based on dogma or authority of any kind, nor on any intuition or revelation, unless indeed it be of the Book of Nature that lies open before our eyes. We need not dwell on the processes of acquiring knowledge by observation, experiment, and inductive and deductive reasoning. The study of scientific method both in theory and practice is of great importance but this topic requires separate treatment. Further it is inherent in the philosophy that the record may be imperfect and the conceptions erroneous; the potential fallibility of our science is not only acknowledged but insisted upon.

Still there are degrees of uncertainty and even the apparent revolution in physics that has occurred in the twentieth century has not so much superseded the older teaching as supplemented and extended it by wider generalisation.

The improvement of knowledge by successive approximations is fundamental and it is worth while to consider some illustrations.

A very simple example of the improvement of a nearly true statement of a relation is that of the Boyle-Charles law. This stated that the volume of a gas is directly proportional to the temperature and inversely proportional to the pressure. It can be expressed as $pv = \mathrm{RT}$. This works very well for most practical purposes but it is not quite correct, especially at higher pressures. In 1873 van der Waals pointed out that the volume (v) taken should be lessened by a quantity (b) related (but not equal to) the volume of the molecules themselves, i.e. $v - b$ for v. The pressure (p) should be increased by a quantity $\frac{a}{v^2}$ which denotes the cohesion of the molecules.

With the expression,

$$\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{RT},$$

a much better agreement with the experimental data was obtained but it was still not absolutely accurate and more or less successful modifications have been suggested.

The 'ideal' gas which obeys the Boyle-Charles law may perhaps be compared to the 'ideal' railway that conforms to Bradshaw. The divergence from the ideal in the performance of a railway becomes greater when the traffic is congested and this factor is not unlike the b of van der Waals. But railways are much worse than gases, chiefly due to the appearance of the human factor, which is quite incalculable.

Where the material can be directly apprehended by our senses, aided perhaps by devices such as cameras, microscopes and telescopes, there can be little doubt as to the validity of the record, though it may need some adjustment of errors of observation and of the value of any magnitude that is involved. The uncertainties are then largely concentrated on the conceptions and generalisations which have been based on the ascertained facts. May I be excused if I refer to the subject I know best in illustration. The molecular structures of the chemist are not directly perceptible but must be deduced from observations on collections of very large numbers of molecules, and yet a most remarkable system has been evolved which even forms the conventional basis of patents and is recognised as valid in courts of law. The degree of uncertainty

is indeed not much increased by the fact that we cannot see these structures.

Leaving aside philosophical dubieties about reality we can say that the limits of our knowledge in this field are somewhat as follows. That the atoms exist as entities, but the problem of the nature and constitution of the atoms themselves has not been fully solved ; rapid progress is being made. We know the arrangement of the atoms in the vast majority of the species of molecules which we find in Nature and we have learned how to construct molecules, even those of a highly complex pattern. The defences of some of the last strongholds are in process of being breached. A brilliant example of modern work of this kind will be described by the President of Section B (Chemistry), Sir Alexander Todd. It concerns the structure and synthesis of the nucleosides, those vastly important constituents of the nuclei of living cells. Even the proteins are being forced to reveal their secrets and though none of them has yet been synthesised we have a foretaste of what is in store in the artificial preparation of the oxytoxic factor of the pituitary by Professor de Vigneaud. And when we speak as chemists of nucleosides, proteins, or the oxytocic factor we are all the time thinking not so much of the specimen in the glass tube but of the way the atoms are joined together in each of the billions upon billions of molecules in the tiniest visible fragment of the material. May I be pardoned by the *cognoscenti*, I cannot forbear from repeating the results of a calculation made by Dr. Shapley, the eminent American astronomer, which illustrates very clearly the huge number of the molecules in any small parcel of matter. In any volume of air four-fifths of the particles are molecules of nitrogen, each consisting of two atoms. If, for example, a deep breath is exhaled the nitrogen molecules in it are rapidly dispersed and after a certain period, probably not more than a few years, perhaps less, they will be equally distributed throughout the atmosphere. The mass of the atmosphere is known approximately and when even distribution has been achieved each unit volume of air at the surface of the earth will contain between two and three of the nitrogen molecules that were present in the original volume distributed. Now to make it more dramatic we select some particular exhalation, preferably a long time back so as to make sure of the necessary even diffusion. Thus we could say that when Shakespeare was writing the first line of the second act of *Hamlet* he exhaled a breath and if you care to inhale a breath now—I sincerely hope you can all do that—it will contain two of the actual molecules exhaled by Shakespeare at that particular time. You would be very unlucky if you only got one and, if you were to capture three, you would belong to a fortunate minority.

The calculation leads to other interesting results. If you exhale a breath now, then, after the lapse of the necessary time, every breath inhaled by every living person on all occasions will contain two of the nitrogen molecules in that breath of yours.

It must be obvious that many of the molecules will be used more than once and the calculation takes no account of the small losses from the atmosphere due to escape, or the activities of Sir Alexander Fleck, among other nitrifying organisms.

The question which is often asked, and ought to be asked is—how can you possibly know anything about the architecture of such exceedingly minute particles? The answer cannot be given in a few sentences but an analogy may help you to appreciate the possibility—again with apologies to those *cognoscenti*. A pure chemical substance is a collection of identical molecules. Let us compare a perceptible amount of it to a crate of identical clocks, each clock then represents a molecule. We reduce the clocks to a heap of metal filings, take a certain weight of these and apply a strong magnet. This takes out the iron, which we weigh and thus we get the percentage of iron in the whole of the clocks, which is also the percentage of iron in one clock. But it may be objected, you have started with the hypothesis that the molecules in a parcel of a pure substance are identical. How can you be sure of that? A short answer cannot be given ; it depends on the recognition that certain properties, for example, many optical properties, are due to the summed effects of individual molecules, and on the elaboration of

techniques for segregating molecules of the same kind. For example, by crystallisation, distillation, diffusion, or by adsorption on surfaces.

Justification of scientific theory has been obtained by the success of predictions and by the unanticipated convergence of more than one line of approach. This is a commonplace of every part of science and the examples to be mentioned could be almost indefinitely extended. In all the cases the degree of coincidence becomes so great as to carry conviction that we have been vouchsafed, if not the whole truth, at least something very closely related to it.

Even the routine predictions of astronomers are a perpetual source of wonder but the circumstances of the discovery of Neptune were sensational. In 1843–4, J. C. Adams, of Cambridge, found that the perturbations of the orbit of Uranus could be fully explained by the existence of an outer planet and he accurately identified the position in which it should be found.

Quite independently, Le Verrier, of Paris, came to exactly the same conclusions and was more fortunate than Adams in securing the quick co-operation of practical astronomers, with the result that Neptune was discovered in the predicted place in the Heavens. It proved to be of the size expected and later was found to follow the expected orbit.

Again, near the beginning of the second decade of the century, Albert Einstein stated that his relativity theory required that the rays of light should be bent under the influence of a gravitational field. He was able to calculate the magnitude of the expected effect. Confirmation came from observations made on the occasions of the two next following total eclipses of the sun. This success was naturally particularly welcome in view of the novelty of the theoretical background and it doubtless accelerated the development of the new physics based on the work of Planck, Einstein and other pioneers.

The death of Einstein has evoked universal tributes of homage to the man and the philosopher. He was undoubtedly one of the greatest of men of science.

A third example of prediction is the successful release of nuclear energy, especially in the so-called hydrogen bomb. We must not allow the horror of the possible misuse of the weapon to blind us to the spectacular triumph of the achievement. It showed very plainly that nuclear physicists had been thinking on the right theoretical lines. Let us hope that they will continue to do so, 100 per cent. and not 99·9 per cent. These matters are shrouded in secrecy but Press reports have suggested that just as the hydrogen bomb is initiated by a Mark-I atomic explosion, so the hydrogen bomb in its turn can initiate a vastly greater uranium explosion. In biological processes energy is taken up in steps by suitable relays to high levels, and an ordinary scientist cannot help wondering whether every possibility of setting fire to the lighter elements has quite certainly been excluded, if nuclear relay is taken to further stages.

Do we really know enough about nuclear reactions to be sure there is no loophole? Has every conceivable case been considered?

The physicists appear to be cocksure that this is so; they claim the 100 per cent. But just as it is necessary that 'justice shall be seen to have been done' I submit that it is desirable that the inner circle of advisers to Governments should not only be right but be 'seen to be right'. This can only be secured by a relaxation of secrecy on the scientific aspects of nuclear energy developments and by some form of international consultation before novel and greatly increased releases of energy are attempted.

The second class of welcome confirmations was the illumination of a topic from two or more distinct sources.

This is now a commonplace of many sciences, for example, again, of organic chemistry. Analytical researches lead to the determination of a molecular structure which may be confirmed, possibly after the lapse of years and in a different laboratory, by synthesis of the substance the molecules of which should possess that structure. If the synthetic product proves to be identical with that isolated from natural sources, the whole system of reasoning both on the analytical and synthetic sides receives important support. Even more striking has been the validation of the conclusions of chemists by X-ray crystallography. At first this led to the confirmation of established structures,

often with some additional information about the arrangement of the atoms in space. Later the X-ray studies were accessory to the first analytic studies. Thus in the case of penicillin, Mrs. Hodgkin and her collaborators were able to fasten upon one of the two alternative structures which the purely chemical researches indicated. Now the chemical and physical methods go hand in hand, with the crystal analysis tending to play a more important role than in the past, and destined to do so more and more in the future.

Another example may be mentioned. The chemical formula for a metal phthalocyanine had the shape of a four-leaved clover and the metal atom was placed at the centre. Robertson's electron-distribution diagram from X-ray evidence had the same discoid shape and the metal atom was where expected. Only a few years ago a special electron microscope cast the clover-leaf shadow of a single molecule of copper phthalocyanine on a fluorescent screen. The necessary apparatus is not particularly complicated and can be purchased.

This was by no means the first time that a single molecule had become apparent to our senses. Owing to the great energy of certain particles carrying an electrical charge, and the invention of devices for amplifying very small currents, we have long been familiar with a variety of methods for observation of the effects of single particles: Crookes's spinthariscope, the Geiger counter, the Wilson cloud chamber and, bringing coals to Newcastle, the techniques developed in such an admirable manner by Professor Powell and his school here in Bristol, for the study of single events at the atomic level; the study of the impact of cosmic rays on the sensitive emulsions of the photographic plate.

In other subjects there are very many examples of the confluence of streams springing from two sources. Examples are the dating of ancient objects by means of radioactive isotopes and the light shed on the movements of races by the hybridisation of plants.

The latter reference is to a hybrid cotton plant found in Peru. There the Porto Rican cotton plant married the Indian and the circumstances are consistent with the view that Indian voyagers reached South America in very early days.

All branches of knowledge are included in the definition of science; always provided that the record has been made without appeal to authority.

There is no philosophical line of demarcation that separates one branch of learning from another though the demeanour of many so-called humanists suggests that they perceive one.

A historian, formerly a colleague of mine, once declared that the absorbing interest of his subject was due to the badness of the evidence, but I think that was only another way of saying that he liked to get his teeth into a difficult problem.

It is not necessary or desirable to pursue this unprofitable discussion; our brother scientists on the Arts side will doubtless work out their own salvation. But it is opportune to add another voice of protest against the often repeated assertion, that 'culture' is the preserve of students of some particular branches of knowledge, and is denied to others. What is meant by 'culture'? If it is the ability to think clearly and to express thoughts in language, what evidence is there that physical and biological scientists lack these qualities?

The acerbity that characterises much of the anti-scientific propaganda strongly suggests that its basis is defensive rather than offensive.

In the meantime these persistent attacks do a great deal of harm by suggesting to young people that they will become automatons if they devote themselves to physical science and that in such a life they will not experience the warmth of human feeling.

What a travesty of the facts this is! The scientific discipline is indeed a hard one but it is most rewarding and full of living drama. Perhaps I may just have time to show one picture from the past. It concerns a beginner in research, but that young man was Louis Pasteur. It will be best to quote his own words from 'Lectures on the Molecular Asymmetry of Natural Organic Products' (1860), reprinted by the Alembic Club of Edinburgh:

> 'When I began to devote myself to special work, I sought to strengthen myself in the knowledge of crystals, foreseeing the help that I should draw from this in my chemical

researches. It seemed to me to be the simplest course, to take, as a guide, some rather extensive work on crystalline forms; to repeat all the measurements, and to compare my determinations with those of the author. In 1841, M. de la Provostaye, whose accuracy is well known, had published a beautiful piece of work on the crystalline forms of tartaric and paratartaric acids and of their salts. I made a study of this memoir. I crystallised tartaric acid and its salts, and investigated the forms of the crystals. But, as the work proceeded, I noticed that a very interesting fact had escaped the learned physicist. All the tartrates which I examined gave undoubted evidence of hemihedral faces.

'Now if we compare the disposition of the hemihedral faces on all the prisms of the primitive forms of the tartrates, when they are orientated in the same manner, this disposition is found to be the same.'

Herschel had already connected the hemihedry of quartz with the right or left rotatory power of the crystals and Pasteur saw that the hemihedry of the tartrates could be an outward and visible sign of the asymmetry of the molecules. Hence he was extremely interested in a note of Mitscherlich of 1844 stating that sodium ammonium tartrate and sodium ammonium paratartrate had identical crystal form and yet only the former was optically active in solution. He thought Mitscherlich must have failed to notice that the paratartrate was not hemihedral.

'I hastened therefore to re-investigate the crystalline form of Mitscherlich's two salts. I found, as a matter of fact, that the tartrate was hemihedral, like all the other tartrates which I had previously studied, but, strange to say, the paratartrate was hemihedral also. Only, the hemihedral faces which in the tartrate were all turned the same way, were, in the paratartrate inclined sometimes to the right and sometimes to the left. In spite of the unexpected character of this result, I continued to follow up my idea. I carefully separated the crystals which were hemihedral to the right from those hemihedral to the left, and examined their solutions separately in the polarising apparatus. I then saw with no less surprise than pleasure that the crystals hemihedral to the right deviated the plane of polarisation to the right, and that those hemihedral to the left deviated it to the left; and when I took an equal weight of each of the two kinds of crystals, the mixed solution was indifferent towards the light in consequence of the neutralisation of the two equal and opposite individual derivations.

'The announcement of the above facts naturally placed me in communication with Biot, who was not without doubts regarding their accuracy. Being charged with giving an account of them to the Academy, he made me come to him and repeat before his eyes the decisive experiment. He handed over to me some paratartaric acid which he had himself previously studied with particular care, and which he had found to be perfectly indifferent to polarised light. I prepared the double salt in his presence, with soda and ammonia which he had likewise desired to provide. The liquid was set aside for slow evaporation in one of his rooms. When it had furnished about 39 to 40 grams of crystals, he asked me to call at the Collège de France in order to collect them and isolate before him, by recognition of their crystallographic character, the right and the left crystals, requesting me to state once more whether I really affirmed that the crystals which I should place at his right would deviate to the right, and the others to the left. This done, he told me that he would undertake the rest. He prepared the solutions with carefully measured quantities, and when ready to examine them in the polarising apparatus, he once more invited me to come into his room. He first placed in the apparatus the more interesting solution, that which ought to deviate to the left. Without even making a measurement, he saw by the appearance of the tints of the two images, ordinary and extraordinary, in the analyser, that there was a strong deviation to the left. Then, very visibly affected, the illustrious old man took me by the arm and said :—

'"My dear child, I have loved science so much throughout my life that this makes my heart throb."

'You will pardon me, gentlemen, these personal recollections which have never been effaced from my mind. In our day, and with our habits, they would offend in a scientific memoir, but they have seemed to me not out of place in an oral account; and perhaps the biological interest of such recollections will constitute one of the advantages of the kind of instruction which the Société Chimique inaugurates to-day.

'Indeed there is more here than personal reminiscences. In Biot's case the emotion of the scientific man was mingled with the personal pleasure of seeing his conjectures realised. For more than thirty years Biot had striven in vain to induce chemists to share his conviction that the study of rotatory polarisation offered one of the surest means of gaining a knowledge of the molecular constitution of substances.'

Thus was the science of stereochemistry born. The main point of Pasteur's work was that he recognised that the optical rotatory power of the tartrates in *solution* showed that the molecules themselves must be asymmetric. The hemihedry of quartz was recognised as due to the asymmetric arrangement of symmetrical molecules in the crystal, like a male, or a female, screw. But the hemihedry of the tartrates, though due in the crystal to the same cause, persisted in the free molecules which were therefore characterised by asymmetry in a certain sense, all right-handed or all left-handed; all male screws or all female screws. A mechanical analogy is very useful here. If a pipe is almost filled with one screw and water is forced along it, the issuing stream will rotate, clockwise or anti-clockwise, according to the nature of the screw. The same thing happens if we pack the tube with small male screws, or small female screws, arranged at random. With an equal number of both kinds there would be no rotation of the issuing water. The single large screw corresponds to the quartz crystal, and the higgledy-piggledy small screws correspond to the freely moving asymmetric molecules of the tartrate in solution.

The philosophy of asymmetry is very intriguing. Only an asymmetric, I should perhaps say dissymmetric, agent can distinguish the right-hand from the left-hand varieties, and of course it was Pasteur himself who sorted out the hemihedral crystals oriented in the two mirror-image forms.

Recently, by a most ingenious application of crystal analysis, Bijvoet has been able to determine the configuration of naturally occurring tartaric acid, which is related to that of many other substances in what we call the L-glucose family. The convention used by stereochemists turned out to be correct, which is quite convenient. The chances for this outcome were naturally no better than even.

Admittedly this example of 'atmosphere' relates to a very exceptional occasion and Pasteur was a very remarkable man, one of the greatest scientists of all time.

But all of us who have been privileged to take part in scientific life and research have had similar exciting and may I add, emotional, experiences in our smaller ways.

The explanation of molecular asymmetry in terms of structure was given in 1875 by Le Bel and independently by van't Hoff. The latter was said by Kolbe "to have no taste for exact science. Mounted on his Pegasus, evidently borrowed from the veterinary stables (van't Hoff taught chemistry at the College of Veterinary Science at Utrecht) he thinks he can see the arrangement of the atoms in space." Nowadays the organic chemist has not completed a synthetic task until the various parts of the molecule have the desired type of asymmetry. Often a succession of screws has to be laid down in the correct order. Thus there are eight places in the cholesterol molecule which can be the source of asymmetric arrangement and that implies 256 possibilities of difference due only to the right- or left-handedness of the parts. And yet the one particular configuration of the cholesterol molecule has been constructed in the laboratory.

The task was equivalent to finding the right road at each of eight successive bifurcations.

Before straying too far from actual science I would like to say something about the use of speculation, which should always arise from some correlation which was hitherto unknown or unnoticed, or the significance of which has been underestimated, or thought to have been underestimated.

In the early stages it may be discouraging to study the literature; one can always find reasons there for believing that a new idea is not worth pursuing, or for that matter for saving oneself the trouble of doing an experiment. Later, having worked up an internal, self-generated pressure of enthusiasm, it is obligatory to face the hard facts already on record. Then, instead of weakly accepting a rebuff we shall at least put up some fight with 'but on the other hand it might be that ——'.

At this point it may be realised that something really has been overlooked by one's predecessors and new crucial experiments must be devised to resolve one dubiety after another. Herein lies the safeguard, the final court of appeal is Nature herself.

The scope of the Association's interest is not clearly defined in our original 'Objects' which I will read, because the early practice of printing them each year has long been abandoned.

'The Association contemplates no interference with the ground occupied by other institutions. Its objects are :—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress.'

Our Sections now include the chief physical and biological sciences, Economics, Anthropology and Archaeology, Psychology, and Education, as well as Engineering, Forestry and Agriculture. There is also an active Division for the Social and International Relations of Science. All this evinces a real catholicity of interest ; omissions are based on practical, rather than on theoretical considerations. For example, clinical medicine and surgery are well served by a number of other organisations.

An important aspect of science is its universality, and its fully international character only rendered possible by the axiomatic freedom it enjoys from authority and dogma. True there have been regrettable incidents such as that of the Misurinian genetics, but the body scientific has acquired considerable immunity against such fevers, which are now all but forgotten.

There is no such thing as British, or German, or Russian, or Jewish science though it is true that the genius of many peoples has enabled them to make characteristic contributions. The French have a special lucidity of mind and expression, as well as elegance of technique, the Americans excel in organisation of research, the Germans in thoroughness, the Japanese in attention to detail, whilst the Jews have imagination and theoretical insight. There is much overlapping of course ; we may find a Memmling among the Florentines.

It has occurred to me to wonder wherein lies our own particular merit. The pioneering spirit of our illustrious forefathers is everywhere acclaimed and there is even now little sign of any decline in our initiative. Can it be at least partly the result of not working so hard that no time is left for reflection ? We certainly defend our leisure from encroachments with special tenacity. Again it may be that we are in a certain sense undisciplined ; but I will not elaborate that suggestion.

Science as a common international possession and the world-wide co-operation in its development must be bulwarks of peace. Here we have a realisable asset of the greatest possible positive value. The brotherhood of scientists is a reality. It is fortunately not unique ; there is a similar fraternity among sportsmen, musicians, chess-players, and other categories of enthusiasts, not encumbered by the so-called ideologies.

It should be clear from the definition that religion in so far as it is authoritarian and dogmatic, can make no contribution to science. But it is no paradox to say that there is room for both science and religion within the mind of a man.

Indeed we have ample proof that this is so from everyday experience. Many of the greatest of scientists have been deeply religious men and eminent divines have been excellent scientists.

Testimony to the help afforded to the Royal Society and to our own Association by churchmen may be found in the records and is still apparent on inspection.

At the Bristol Meeting of 1836 the Presidents of Sections (A), (B), (C), and (D) were all clergymen.

On the other hand many scientists have been, and are, free-thinkers. Yet, the two groups should be united in the unflinching pursuit of the truth and in a spirit of tolerance.

Titles such as 'Science and Religion' tend to confuse the simple issue which relates to 'Scientists' and not to 'Science'. Again the uncertainties of modern physics are now considered by some to justify mystical analogies.

It would appear that there are many who feel an irresistible urge to fill up all gaps in knowledge by their own inventions. Such intuitive speculation can indicate useful working hypotheses but it is not science *per se*.

Actually physics is a much more complete and satisfactory system now than it was in the later nineteenth century. It is true that we are perplexed by many new problems, but at least we are aware that they exist. That is an improvement on complacency founded on ignorance. The human mind can be shown to be inadequate in several ways. Speaking subject to correction by philosophers it would seem that certain alternatives that are logically mutually exclusive are equally inconceivable by us. The conceptions of a continuum or a discontinuum, of creation or indefinite projection of reality into the past cannot be grasped by the human mind. Yet the fact that we find something to be incredible is no proof that it does not exist. Hence physicists are content to derive expressions for the description of phenomena with the minimum of extraneous hypothesis.

Fifty years ago an ether was an article of faith ; there could be no action at a distance without an intervening link, electro-magnetic waves must occur in something, and so on. But each invented ether was about as incredible as the absence of one. Nearly fifty years ago I remember being fascinated by Osborne Reynolds's ' Inversion of Ideas as to the Structure of the Universe.' This postulated a very dense ether consisting of minute grains. Matter was a negative inequality in this ether, that is, an absence of grains, a kind of hole. It could be shown that the inverse square law would govern the attraction of such holes. Mathematical physicists have evidently found it hard or impossible to use this theory, but I well remember thinking more or less as follows : Yes, but what are the grains made of? They have been invested with some of the properties of matter and they are a kind of engineer's dream of perfect Daltonian atoms on a very small scale. The argument is quite inconclusive, but I found then, and have always found since, that the idea of a continuum, even inside a grain of the ether was, and is, repugnant. So nowadays we just arrange to do without this comfortable resource of an ether, though I have been told that Professor Dirac may revive the idea in some form or another—which few of us will understand.

Another feature of the complex make-up of the scientist is that he is a citizen and owes a duty to the State.

This leads to obvious conclusions in most respects but where do we stand in regard to the invention of military weapons ?

In war-time few of us entertained any doubts about this ; it was our clear duty to help our country to the best of our ability. Now it is a matter for the conscience of the individual, taking into consideration that we have not yet achieved a real peace.

The position is one of unstable equilibrium and it still seems that as citizens we should do all in our power to strengthen the hands of our leaders, in whom we have placed our trust, and who we believe are sincere in their desire to achieve universal peace among the nations. Even the terrible nuclear-energy weapons must be developed by our scientists in peace-time so as to build up the necessary potential of deterrence, our only defence against extinction.

I have mentioned only two or three of the personal problems of scientists but there are, as is very well known, many national questions affecting scientists and on these I can say very little in this Address. The chief is perhaps the training of scientists which starts in the Schools and involves the supply of science teachers. This matter has been raised in the General Committee here in Bristol. We hope to make some small contribution to a solution of difficulties that are the cause of considerable justified anxiety among industrialists.

Finally, I want to say a word about technology and technologists. Dr. Holmyard and Professor Singer have, in the first volume of their monumental work on the History of Technology, pointed out that it all started with handicraft. The modern technologist is still a man with know-how and can-do, whether he be technician, craftsman, designer or supervisor, and he is equipped with knowledge of general and special scientific and engineering subjects.

It is generally agreed that we have far too few technologists in this country and that our economic survival is improbable unless we take some drastic steps to remedy

the situation. The proposals that have been made are admirable as far as they go, but, in the opinion of many who are well qualified to judge, they do not go nearly far enough.

It is doubtful whether any scheme tied to existing Institutions can meet the requirements and to come right down to ground level. I suggest that we need a 'Technology Grants Committee', independent of any other body and with power to expend a very considerable sum of money.

This idea has probably already been adumbrated. In which case I think it should be supported.

I must tender humble apologies for the attempt to cover so many topics, and so inadequately. At least I have avoided for you the dullness of which King Gama complained. There *will* be plenty of things to grumble at.

[The President may add some statement on an Appeal which is being drafted relating to the dangers of thermo-nuclear weapon warfare, but this cannot be printed in advance due to the discussions, manifestos, and conferences which will transpire in the interval before the Meeting.]

PHYSICS OF THE SOLID STATE

Address by

PROF. N. F. MOTT, F.R.S.

PRESIDENT OF SECTION A

THE physics of the solid state is a very wide subject for treatment in a lecture lasting one hour. It is a very fashionable subject; after nuclear physics—on which Sir John Cockcroft spoke at the last meeting of this Association—research on the physics of the solid state probably occupies the greatest number of physicists and produces the largest number of contributions to the scientific journals. It ranges from the most difficult and abstract subjects far away from practical applications, such as the behaviour of the electrons in a metal and the reason why at very low temperatures the electrical resistance of some metals drops away to nothing at all, to problems of immediate technical importance such as the development of crystal valves and the effect on the materials of a nuclear reactor of heavy bombardment by neutrons.

In this lecture the last thing that I want to do is to survey the whole field. That would make my lecture into just a catalogue. What I hope to do is to take one or two problems, to show their importance and interest and to explain how for their study it is necessary to use techniques drawn from other branches of science, such as nuclear physics.

I shall start my lecture by introducing you to the problem of self-diffusion in solids, particularly in metals. I want especially to talk about the concept of a vacant lattice site in a crystalline solid, and in fact the whole lecture will be concentrated round this theme. I will show how it affects ordinary processes of metallurgy, the behaviour of materials in reactors and finally the relation it may have to the urgent and important problem of metallic fatigue. I think that a discussion of this problem will show how these techniques from other subjects get drawn into the physics of the solid state, and also serve as an introduction to these other problems of perhaps wider interest.

Everyone knows that if you put two metals together and heat them sufficiently, they will weld together, an intermediate region forming occupied by an alloy of the two metals. This means that when the crystal lattice of a metal is occupied

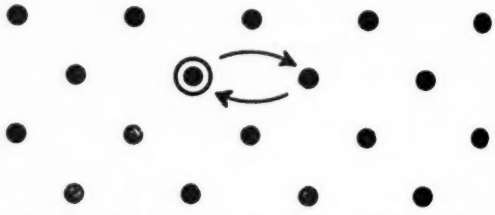


Fig. 1.—Showing the exchange of two atoms in a crystal.

by two different kinds of atom, as in Fig. 1, it is possible for them to change places. Moreover, the higher the temperature the more frequently they will change places. And if two atoms of different kinds can change place, one can say a fortiori that atoms of the same kind can change their places too. This process is known as self-diffusion. We could ask several questions about it; first, at a given temperature, how frequently do a pair of atoms change places? Secondly, what is the mechanism by which they change place; and thirdly—since the exchange of a pair of exactly similar atoms can hardly alter the properties of the metal—of what interest is a study of self-diffusion? I will deal with these in turn.

As regards the means of measurement of self-diffusion, the most direct method is to plate or otherwise deposit a radio-active isotope on to the surface of a metal and by measuring the activity of the material

underneath to determine the rate at which the isotope penetrates into it. The radio-active isotope is of course chemically just the same as the surrounding metal and the time required for a radio-active and a stable atom to change places is just the same as for two stable atoms. Experiments of this type were first carried out by Hevesy for lead for which a radio-active isotope (radium D or thorium C) can be obtained from the decay of radio-active materials occurring in nature. Since artificial radio-active isotopes have become available, a great deal of work of this type has been done. Table I gives some values of the ' exchange ' time, or time necessary for a pair of atoms to change place, for copper at various temperatures. These experiments have brought out one interesting point, not shown in the table. Diffusion is much quicker along the grain boundaries than in the bulk of the metal—hardly a surprising fact, but one which necessitates some care in interpreting experiments.

TABLE I

Frequency with which atoms in metallic copper change places

Temperature, degrees Centigrade	0	400	650	850	1030
Frequency (sec.$^{-1}$)	10^{-22}	1	5×10^{3}	5×10^{5}	5×10^{6}

There is another way in which it is possible to measure the rate at which atoms change their places, without using radio-active isotopes, but applying another property of nuclei, the ' nuclear spin.' It will, I imagine, be familiar to most of my listeners that the atomic nucleus has a very small magnetic moment, varying of course from one nucleus to another but of order of magnitude about one thousandth of the magnetic moment of the electron. In other words, each atomic nucleus is a small magnet. But in spite of its smallness, the moment or strength of the magnet can very easily be measured, by the now famous method of ' magnetic resonance ' developed particularly by Bloch in Stanford. The idea of this method is as follows. A piece of some material, containing the atom to be investigated, is placed between the poles of a magnet. The magnets will then precess round the field, with a frequency—the Larmor frequency, which depends on the strength or moment μ of the nuclear magnet, the strength H of the magnetic field, the mechanical moment of the nucleus and so on. The formula is written

$$\nu = \gamma H/2\pi$$

where γ is called the gyromagnetic ratio of the nucleus. For reasonable fields, this gives frequencies about the same as those of radio waves of high frequency. So if the material is subject to a radio wave, or is placed in a cavity in which high frequency waves are excited, what happens ? If the frequency is just right—and only then—the radio wave is strongly absorbed. It is thus possible to measure the frequency of this Larmor rotation.

The way in which this method is used to measure the rate at which atoms change places is as follows. One measures the breadth of the absorption—that is the range of fields H at which the radio wave is absorbed. You might ask, why is there any breadth at all ? Well, the reason is that any atomic nucleus will be acted on not only by the magnetic field from the big magnet but also by the much smaller

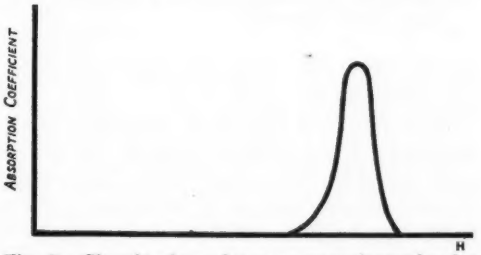


Fig. 2.—Showing how the resonance absorption by nuclear spins depends on the magnetic field H.

fields from all other nuclei—which you remember are magnets themselves. Small though these magnets are, they are very close to each other, and may produce a field of say 2 gauss, ten times the earth's field. Moreover the direction of this field will be random. So the resonance, the range of fields ΔH in which the absorption occurs as you change the field H, should be spread over a range of about 2 gauss.

But suppose now the atoms are changing places, and that they change several times in the period in which the nucleus precesses. Then the situation is quite different. The disturbance of any one nucleus due to all the other nuclei keeps on changing, so that the average effect, averaged over one cycle, becomes smaller and smaller as the frequency of interchange increases. The line therefore gets much sharper.

Fig. 3 shows some results due to Gutowski. It will be seen that the line-width falls between 160° and 200° K, and one can deduce that at about 180° K the jump frequency is about the same as that of the radio wave used. It is interesting that by this method one can measure the rate at which absolutely similar atoms change

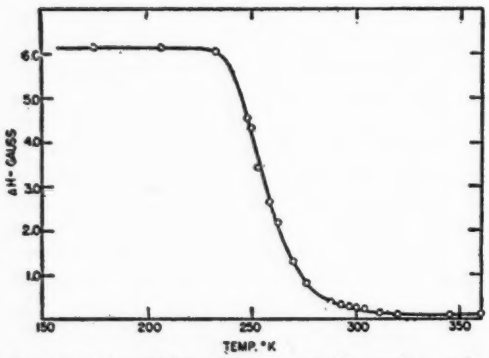


Fig. 3.—Line-width ΔH in gauss for nuclear spin resonance in sodium, plotted against temperature (after Gutowski).

places, a process which does not affect the properties of the solid at all.

You will see that two methods, both borrowed from nuclear physics, are available to determine the jump frequency. Where comparisons have been made, they agree pretty well. Let us now ask, how the atoms jump. Two hypotheses are available. One is that two atoms move at once, as sketched in Fig. 4 (a); the other is the so-called vacancy hypothesis, which is now very widely accepted. According to this hypothesis a crystalline solid in equilibrium at some temperature T contains a certain proportion of vacant

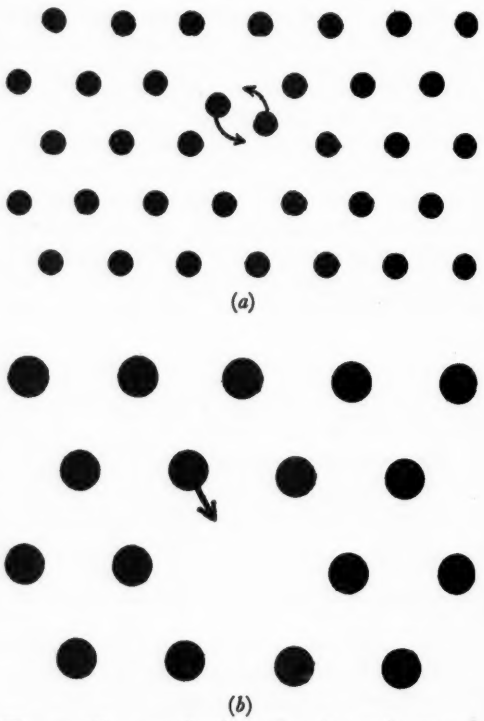


Fig. 4.—Two mechanisms for the exchange of atoms.

lattice sites. These are, simply, positions from which an atom is missing from the lattice. These vacancies are thought to be formed at the surface of a solid and to diffuse inwards, just as atoms or molecules evaporate from the surface of a solid and form a vapour. At any temperature T one supposes that there are in the solid a certain number of these vacancies ; the proportion of sites unoccupied will be roughly given by the term $\exp(-W/kT)$, where W is the energy necessary to form a vacancy. Using plausible values of W, the values of this factor are given in Table II on the following page.

One supposes too that the vacancy moves about in the crystal, the number of times that it moves from one lattice position to another being given by a factor of the

TABLE II

Proportion of vacant sites

	W or U(eV)	0°C	100°C	M.P.
Copper . . .	0·9	2×10^{-17}	$6{\cdot}5 \times 10^{-13}$	$4{\cdot}5 \times 10^{-4}$
Lead . . .	0·5	$5{\cdot}5 \times 10^{-10}$	$1{\cdot}5 \times 10^{-7}$	6×10^{-5}
Jump frequency for a hole (in sec^{-1})				
Copper . . .	1·1	5×10^{-9}	$1{\cdot}5 \times 10^{-3}$	8×10^{7}
Lead . . .	0·7	10^{-1}	$3{\cdot}5 \times 10^{2}$	$1{\cdot}5 \times 10^{6}$

type $\nu \exp(-U/kT)$, where ν is an atomic frequency (c. 10^{12} sec^{-1}). Values of this factor are also given in Table II. The hypothesis is, that an atom will only move from one site to another when a vacant lattice site is next to it. So the number of times per second that an atom moves is

$$\nu \exp\{-(W + U)/kT\}$$

Why now do we favour the vacancy method of diffusion, instead of the more direct process in which two atoms change places? There are several reasons: in the first place, from our knowledge of interatomic forces, it is possible to make some estimate of the energy required to activate the two processes, and that for the vacancy mechanism is the lower. Then there is the Kirkendall effect which shows that in alloys during diffusion marks or wires embedded in the material drift about. But perhaps the most direct evidence is that of the work in which vacancies are produced artificially and are found to speed up diffusion.

One way in which vacancies can, we believe, be produced artificially is by bombarding the material with fast particles, either neutrons, protons or electrons. If enough energy is transferred to one of the atoms of the solid—in practice an energy above 25 eV is required—the atom will be knocked out of its normal position into an interstitial position, producing thus a vacancy and an interstitial atom. So vacancies—as well as interstitials—are produced artificially; and one would expect diffusion or self-diffusion to be greatly increased thereby.

I may say at once that certain experiments, especially those carried out at Harwell, show that this is so. But I do not want to spend time on this, but would rather pass on to the general question of radiation damage, which the development of nuclear reactors, in which the materials are continually subject to heavy radiation, has made an important and fashionable subject of research.

You will realise that many substances, silver bromide, for example, or living tissue, change their character under the influence of any ionising radiation, ultra-violet or even visible light. It is not of this type of damage that I want to speak. It is the kind of damage that I have described above, in which an atom is knocked out of position by a charged or uncharged particle. This is the only kind of damage with which we are concerned in metals and probably also in graphite, which after all are some of the most important substances in uranium reactors. The effect on the uranium itself is technically the most serious, the metal swelling and blistering as the light isotope is used up, though here the observed behaviour may well be partly due to the presence in the metal of fission products, some of them gases. In this lecture I would like to limit myself to the direct effects of displacing atoms. These effects can be quite big; one estimates that one fast neutron will knock 6,000 aluminium atoms out of position and one fission fragment 25,000 uranium atoms. If you leave a metal in a reactor long enough you ought to destroy the crystalline structure completely, and

exposures for a few weeks or months will displace one atom in ten thousand.

The effects of radiation damage are various. As I have said, after irradiation the diffusion of one metal in another is accelerated, and this does seem to indicate that vacancies are indeed formed and that they can move about in the metal, and that the vacancy model of diffusion is the correct one. Also the strength and density are affected. A property much investigated, on account of the ease with which small changes can be detected, is the electrical resistance. Just as impurities increase the electrical resistance of copper or of most other metals, presumably because the presence of vacancies or of interstitial atoms act just like impurities, so the electrical resistance provides a good measure of the damage to which a metal has been subjected.

We had hoped that the investigation of electrical resistance and its recovery when a metal is heated would give us some clear indication of how vacancies and interstitials move. As one heats the metal up, either the vacancies or the interstitials will begin to move first. One could expect a simple bimolecular reaction, the number n of either type decreasing according to the law

$$\frac{dn}{dt} = -Cn^2$$

So, while self-diffusion depends, let us say, on the number of vacancies present and hence on the energy required to form them, *and* on the speed with which they move (the product of the two), the recovery of the resistance should depend only on the speed at which they move. One might hope to obtain by these methods the speed of movement. But, unfortunately, the observed facts are much more complicated ; recovery occurs over a wide range of temperature, showing several activation energies. Doubtless impurities in the metal will have much to do with this since interstitial atoms and vacancies may well be strongly trapped at impurities ; doubtless also the interstitial atoms and vacancies do not disappear only by recombining, but also by migrating to grain boundaries and dislocations. It has also been suggested that the intense heating produced by bombardment produces regions of local disorder much more complicated than vacancies and lattice defects, the so-called thermal spikes. A good deal more research will have to be done before we can sort out these effects for certain.

The interstitial atom, too, presents a problem. In a metal with a close-packed structure like copper or aluminium there must be very little space between the atoms, and an atom which is violently forced into an interstitial atom by a blow from a neutron must distort its surroundings considerably. Fig. 5 shows the results of rough calculations of the amount of distortion expected. It would aid in sorting out this complicated set of phenomena if it were possible to calculate the

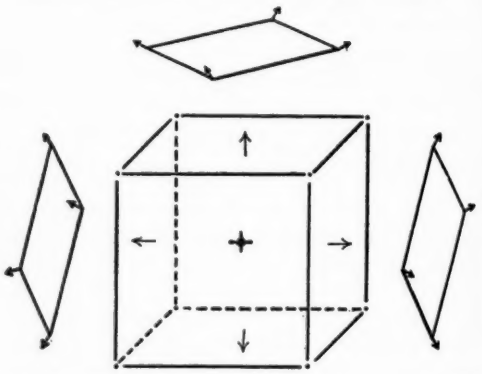


Fig. 5.—Displaced atoms round an interstitial atom in copper.

degree of distortion, but the difficulty here is that we have little knowledge of the repulsive forces between atoms when they are pressed very close together. The pressures available in the laboratory are not high enough to make it possible to deduce this information from p–v curves. It is possible that we may get some information either from geophysical data, or from the properties of metals in contact with detonating explosives, as in some experiments recently published from the Los Alamos Laboratory (Walsh and Christian, *Phys. Rev.* 97 (1955) 1544).

But now I would like to turn to rather wider issues. You may wonder why I have been so concerned with what may seem to you a very small corner of physics —this idea of vacant lattice sites, that is to say little holes, wandering round in the

interior of crystalline solids. The reason is the following : certainly one of the most important branches of the physics of the solid state is that which is concerned with the working of metals and with the strength of metals and of other solids too. The working of metals and their tempering and hardening is one of the oldest arts of man and as science has progressed, scientific methods have been applied to it. But it is only since the last war that we have begun to have any real understanding of what happens, in terms of the motion of atoms, when a metal is deformed. I think that this concept of holes has a great deal to do with it.

Now it is common knowledge that when a metal is deformed it becomes harder. It is a recent discovery, also, that when a metal is deformed, vacant lattice sites and interstitial atoms are formed within it. We can deduce this, mainly, from the effect on the electrical resistance. The curious thing is that the vacancies and interstitial atoms do not seem to have any effect to speak of on the mechanical strength of the cold-worked metal. When you heat the metal, most of the extra resistance disappears, and it does it at just about the same temperatures as would be necessary to get rid of the extra resistance in a metal which had been subject to bombardment. In fact this is the best bit of evidence that one *is* producing vacancies and interstitials. But to soften the metal you have to heat it to much higher temperatures than are necessary for getting rid of the vacancies—and in fact we believe the hardness in a cold-worked metal is due to another type of imperfection altogether, the dislocations.

I do not want to go into the mechanism by which these vacancies and interstitials are produced in cold-worked metals ; this has been discussed a lot lately, we are not sure about it, but we have some theories. It seems reasonable to suppose, however, that they are formed where the deformation is greatest. Now it is very well known that deformation in metals is by no means spread uniformly throughout the crystal grains of the metal, but concentrated in ' slip zones,' which mark the surface of the metal, giving the slip lines visible under the microscope, and which occupy perhaps a tenth or even much less of the total volume of the metal. So it is here that one might expect these interstitial atoms and vacancies to form. Rough estimates would suggest that within the slip bands one atom in a thousand is pushed out of position, as many as would be displaced in a very intense bombardment with neutrons.

What now would be the consequence of the presence of so many vacancies and interstitial atoms in the slip bands ? The first consequence is that self-diffusion and indeed diffusion of other impurities will be greatly speeded up, as it is in a substance that has undergone bombardment. It certainly does seem to be the case that precipitation in supersaturated alloys does occur preferentially along the slip bands ; an example is shown in Fig. 6, due to Dr. J. Nutting working in the Department of Metallurgy of the University of Cambridge. They show electron micrographs of aluminium-based copper alloys containing about 4 per cent. of copper, quenched and held at 200° C. to allow precipitation. The lines on Fig. 6 (a) are thought to be slip lines formed by the strain set up in quenching. It will be seen that they contain fewer and larger precipitates, which is just what one would expect if diffusion was more rapid, owing to the presence of vacancies in the slip band. The contrasting situation when dislocations are present near the surface of the metal, which act as nuclei on which precipitation can occur with high probability, is shown in Fig. 6 (b).

However, all this evidence is perhaps not conclusive ; certainly slip lines may also contain dislocations, and it is always possible that their effect, as nuclei on which precipitates can form, is predominant, though the contrasting effect shown in these two photographs speaks in the opposite sense.

I would like to turn now to another possible effect of the vacancies and interstitials, more speculative but possibly more important. This is the phenomenon of fatigue. There is to be a paper on this subject later today, and I do not want to anticipate what will be said there. But it is common knowledge that metals break after sometimes tens of millions of applications of an alternating stress ; that the ' crack ' first forms quite early on in the

test and slowly spreads, and that it first forms along a slip line. Photographs suggest that, under repeated application of a stress, slip lines formed early on will gradually broaden, and then turn into cracks. An example is shown in Fig. 7. Moreover, the process usually starts on the surface and is accelerated by the presence of air; though recent experiments at Oxford at very low temperatures show that fatigue fractures can occur at the very lowest temperatures, when the presence of gaseous oxygen is altogether impossible.

Now it seems extremely probable that the material in a slip zone remains rather prone to slip; in other words each time the stress is reversed slip occurs one way or the other—not of course a very big displacement, probably nowhere near the whole distance that the material slipped the first time, but if the stress is big enough at any rate to a certain extent. To explain why would entail a discussion of the theory of dislocations, which I want to avoid in this lecture. But clearly a slip band is a region in which slip is easy, and there is much evidence that after deformation in one direction some slip in the opposite direction can occur for quite small stresses.

Now if in a fatigue test slip is occurring backwards and forwards in both directions in some of the slip bands, there should be a continuous production of vacancies and interstitials in these slip-band regions. One can make a very rough guess at how fast they are produced; perhaps after 10^5 cycles every atom will have been displaced. So the effect in the slip band is really expected to be very similar to that of intense neutron bombardment. The material will become gradually disordered, more and more atoms being pushed out of their proper positions.

Now it is not, of course, clear that an intense disordering of the crystal would lead to fracture—and here we are certainly in an unknown region because one really does not know what happens when you stir up a lattice completely. But one can hardly escape the conclusion that the material would expand locally, that great stresses would be set up and local cracks result.

This is all very speculative; a theory of fatigue in terms of atomic movements is yet to be worked out. But I hope that I have shown you that this concept, that of a vacant lattice site in a crystal lattice, has very wide applications in physics. It is something which moves about in a metal and which makes the exchange of atoms possible; using nuclear spins or radioactive isotopes one can find out how often it enables atoms to change places; they can be produced by bombardment with fast nuclear particles; and finally—I believe—lots of them are produced in metals when subject to alternating stress, and they may be the cause of the failure in fatigue.

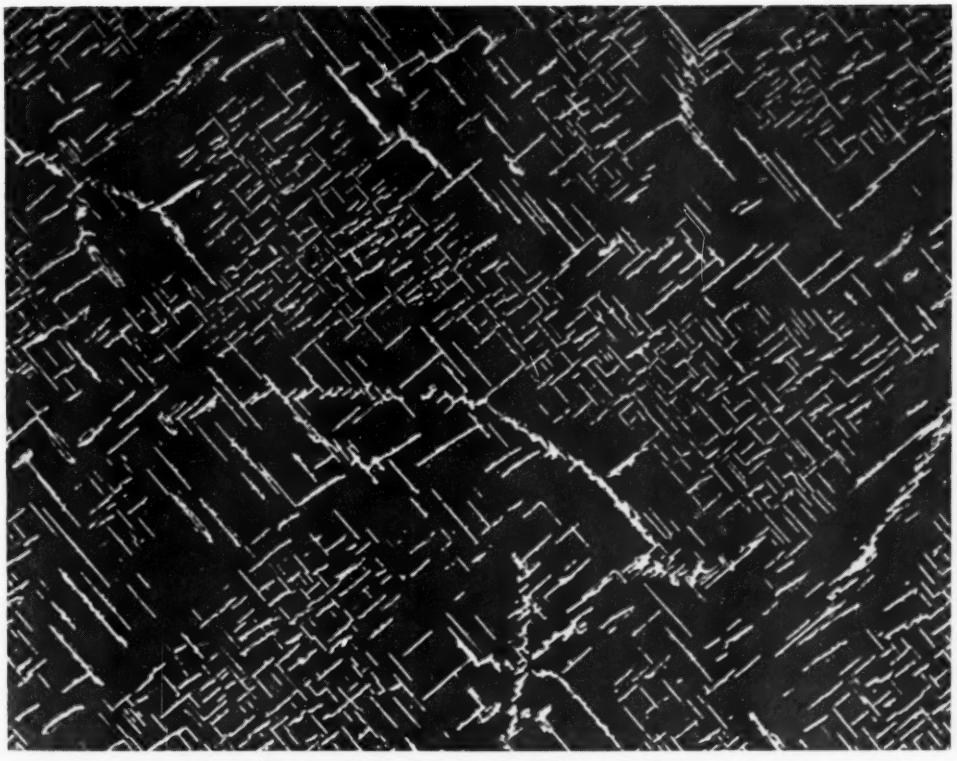


Fig. 6.—(*a*) Precipitation in AlCu on slip lines.

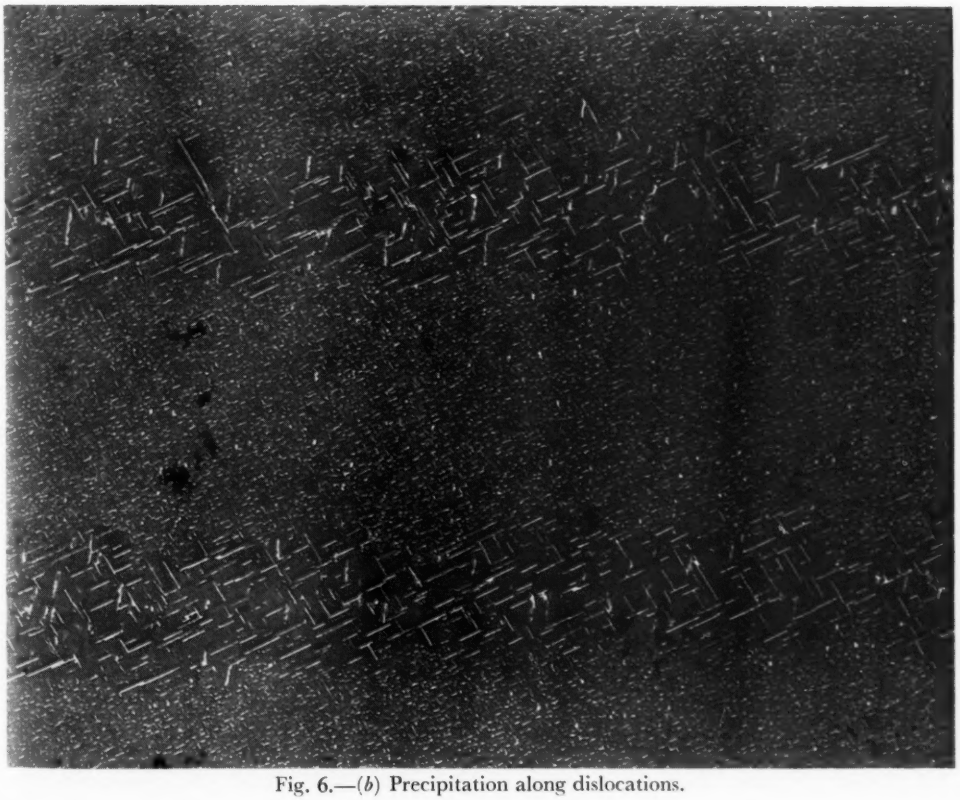


Fig. 6.—(*b*) Precipitation along dislocations.

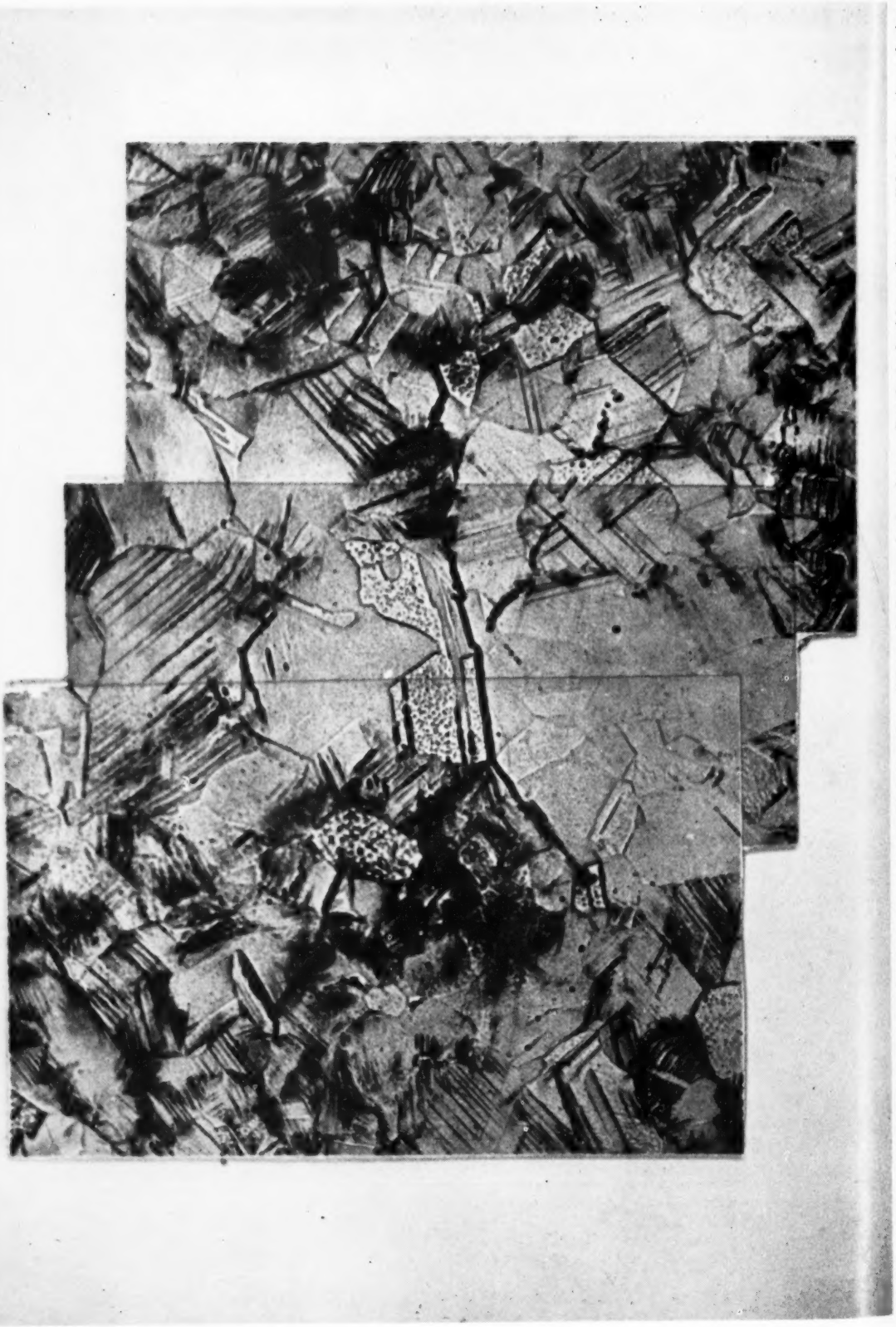


Fig. 7. Mosaic of copper polycrystal, broken in fatigue after $3{\cdot}65 \cdot 10^6$ cycles. The main fracture is elsewhere. Specimen had been electro-polished three times during

NUCLEIC ACID STRUCTURE AND FUNCTION

Address by

SIR ALEXANDER TODD, F.R.S.

PRESIDENT OF SECTION B

THE term nucleic acid applies to a group of macro-molecular substances found in all living matter which, on hydrolysis, yield a mixture of heterocyclic bases, a sugar, and phosphoric acid. They occur, at times, free, but more often in association with proteins as the so-called nucleoproteins. These, probably the most complex of all substances of biological origin, include enzymes and viruses and are of profound importance in health and in disease. The advance in chemical knowledge of the proteins has proceeded steadily—if at times slowly—over a long period, but the chemistry of the nucleic acids remained until recently a neglected field of study. True, since the discovery of the first nucleic acid more than eighty years ago, investigation has gone on sporadically, but it was often rather superficial and the results were frequently misleading. It is worthy of note that most advanced textbooks of organic and biochemistry published more than ten years ago dismiss the nucleic acids in a page or so as relatively straightforward tetranucleotides, giving structural formulae for them which are wrong in almost every detail and which were based on evidence which was, for the most part, quite invalid. All this has changed, for the past five years have seen an astonishing revolution in the field and one which has put our knowledge of nucleic acid structure on a firm foundation and brought it to a point where it stands comparison with our knowledge of the proteins.

The natural nucleic acids are of two types—the ribonucleic acids which yield on hydrolysis, the sugar D-ribose and the deoxyribonucleic acids which yield 2-deoxy-D-ribose. The old idea that there were only two nucleic acids in nature, one—deoxyribonucleic acid—occurring in animals, and one—ribonucleic acid—in plants, is long since exploded and it is realised that there are a great number of them, all of which, however, belong to one or other of the two types mentioned. The broad picture of nucleic acid structure emerges clearly from a study of their hydrolytic degradation which follows the scheme :

Nucleic Acid
↓
Simple Nucleotides
↓
Nucleosides + phosphoric acid
↓
Purine and pyrimidine bases + D-ribose or 2-deoxy-D-ribose

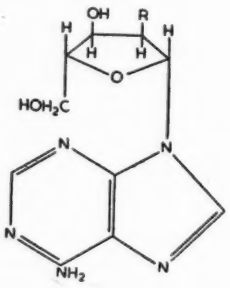
The nucleic acids are thus polynucleotides consisting of a large number of simple nucleotides linked together, the latter being phosphoric esters of the nucleosides. The nucleosides themselves are ribosides or deoxyribosides of heterocyclic bases of which two (adenine, guanine) are purine, and five (uracil, thymine, cytosine, 5-methylcytosine and 5-hydroxymethylcytosine) are pyrimidine derivatives. It may be that very small amounts of other bases occur in certain nucleic acids, but those mentioned may be regarded as the well-established components. From the ribonucleic acids four nucleosides, adenosine, guanosine, uridine and cytidine, are obtained, and from the deoxyribonucleic acids six, of which four (deoxyadenosine, deoxyguanosine, thymidine and deoxycytidine) are widespread and two (5-methyldeoxycytidine and 5-hydroxymethyl-deoxycytidine) are limited in occurrence. Nearly thirty years ago Levene and Simms showed by titrimetric studies that in the nucleic acids the individual nucleoside residues were linked together by phosphate groupings and excluded the

Fig. 7. Mosaic of copper polycrystal, broken in fatigue after 3·65 10⁶ cycles. The main fracture is elsewhere. Specimen had been electro-polished three times during

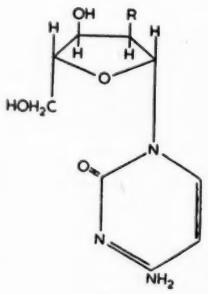
possibility of ether or pyrophosphate linkages. This evidence has been confirmed by all later work and we can consider the nucleic acids as poly-phosphodiesters. There is no convincing chemical evidence for any other type of linkage in the molecule.

Any understanding of nucleic acid structure must rest upon a knowledge of the detailed structure of the nucleosides and nucleotides and of the position of the internucleotidic linkage. It is with these matters that my colleagues and I have concerned ourselves in the last eleven years at Cambridge, building on the earlier studies of Levene and others, and applying the standard methods of organic chemistry, as well as a variety of new experimental techniques which have made it possible to handle groups of otherwise most intractable materials. It would be out of place in a general lecture such as this to enter deeply into the detail of this work, despite its considerable organic chemical interest. Instead, I shall attempt to summarise it, stressing those aspects which are directly relevant to the derivation of general structural formulae for the two types of nucleic acid.

Taking first the question of nucleoside structure ; it was shown conclusively that the natural ribonucleosides are uniformly β-D-ribofuranosides, the sugar being attached at N_9 in the purine, and N_3 in the pyrimidine nucleosides, and the conclusions were verified by total synthesis. Typical examples of these ribonucleosides are adenosine (I ; R = OH) and cytidine (II ; R = OH). Although they have not yet been synthesised, the deoxyribonucleosides have similarly been shown to be 2-deoxy-D-ribofuranosides with the sugar residue at N_9 in the purine and N_3 in the pyrimidine nucleosides. That the configuration at the glycosidic centre is β has been proven experimentally for deoxyadenosine (I ; R = H), thymidine and deoxycytidine (II ; R = H) ; in the case of deoxyguanosine the inferred β-configuration has not been rigidly proved although it is almost certainly correct. An interesting absolute method of configurational determination has been used in this field. It depends on the formation of *cyclo*nucleosides from the 5′-tosyl derivatives of the nucleosides ; in these *cyclo*nucleosides, ring closure occurs between $C_{5'}$ in the sugar residue, and N_3 in the purine or the oxygen at C_2 of the nucleus in the pyrimidine nucleosides. Their formation is only possible if the configuration at the glycosidic centre is β. Such *cyclo*nucleosides are of interest in various directions, but whether they have any significance in biological processes is unknown.



I

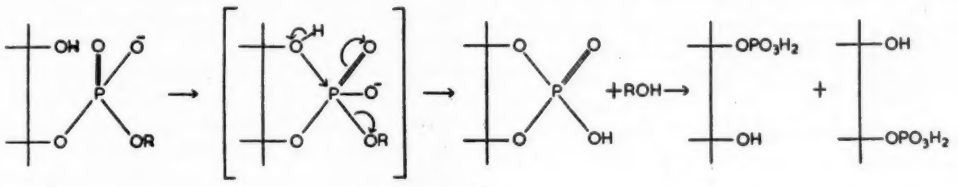


II

From each ribonucleoside three, and from each deoxyribonucleoside two simple nucleotides can be derived. All of these have been synthesised using one or other of several new methods of phosphorylation, the most widely employed being that which uses dibenzyl phosphorochloridate $(C_6H_5CH_2O)_2POCl$ as phosphorylating agent. By using appropriately protected intermediates it was possible to provide unambiguous synthesis of the ribonucleoside-5′ phosphates and the deoxyribonucleoside-3′ and -5′ phosphates. Unambiguous synthesis of the 2′- and 3′-phosphates of the ribonucleosides proved a much more difficult problem partly because of the difficulty of preparing suitable protected intermediates and the element of uncertainty introduced by phosphoryl migration. True, the 2′- and 3′- phosphates were obtained by Brown and Todd by phosphorylating the 5′-trityl derivatives of the ribonucleosides and they could be separated by ion-exchange chromatography, but it could not be said from the method of preparation which was the 2′- and which the 3′- compound.

It was at about this stage in the chemical studies that Carter and Cohn (1949) in the United States applied the method of ion-exchange chromatography to alkaline hydrolysates of ribonucleic acids,

and showed that contrary to previous belief, not four, but eight simple nucleotides were to be found in them. These



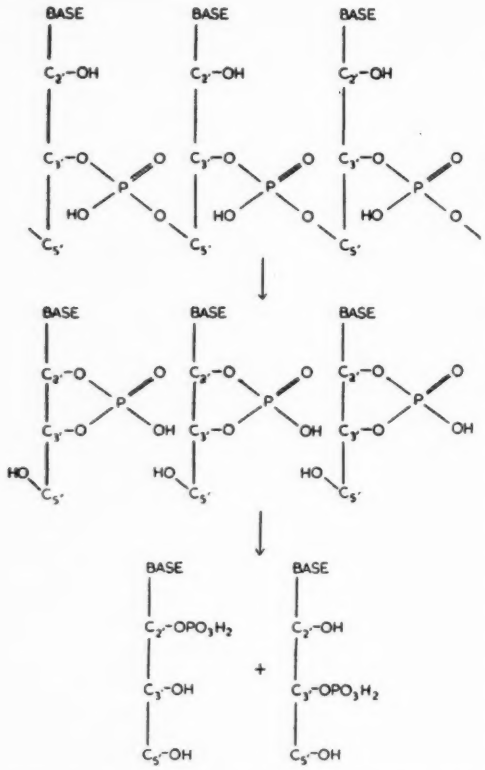
III

eight were made up of four pairs of isomers—the *a* and *b* nucleotides—each pair corresponding to one of the four nucleosides. It was soon established that the *a* and *b* nucleotides were identical with the 2′- and 3′-phosphates of Brown and Todd, but the problem of their true orientation remained. The general theory of nucleic acid structure and breakdown was advanced and developed before this point was resolved, but it will be perhaps simplest if we introduce at this point the results of more recent studies which have shown conclusively that the *a* nucleotides are ribonucleoside-2′ phosphates and the *b* nucleotides are ribonucleoside-3′ phosphates. It is worthy of note that in proving that uridylic acid *a* is uridine-2′ phosphate, an ingenious use was made of *cyclo*nucleoside formation which occurs between $C_{2'}$ and O_2 in uridine with inversion of configuration so that on opening the *cyclo*nucleoside hydrolytically, uracil β-D-arabofuranoside is obtained.

Examination of the synthetic ribonucleoside-2′ and -3′ phosphates showed that although stable in alkaline solution, they underwent phosphoryl migration in acid solution, either isomer giving the same equilibrium mixture of both. Even more interesting was the behaviour of their mono-esters. Although diesters of phosphoric acid are normally resistant to alkaline hydrolysis, mono-esters of the ribonucleoside-2′ or 3′-phosphates are very labile to alkali and hydrolyse giving in every case a mixture of the free 2′- and 3′-phosphates, the cyclic 2′ : 3′-phosphates being detectable as intermediates. This lability, which is found also in esters of the glycerophosphoric acids, is undoubtedly a feature of all phosphodiesters in which a *cis*-hydroxyl group is situated in the vicinal position to the phosphate. The following mechanism is confirmed by kinetic studies :

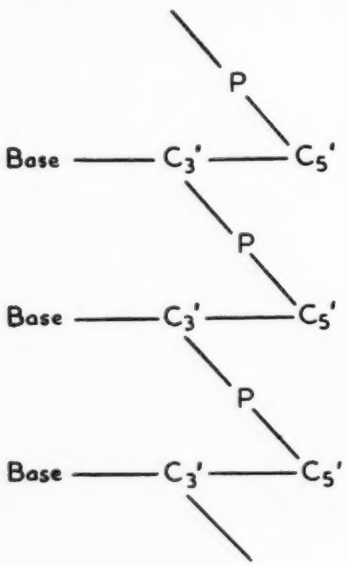
Based on these studies on the hydrolysis of the nucleotide esters, Brown and Todd (1951), advanced an interpretation of the hydrolytic behaviour of the nucleic acids and, resting on it, a general theory of their structure which has since become generally accepted and which agrees with all the known facts. It has long been known that ribonucleic acids are labile to alkali, undergoing complete disruption into simple nucleotides ; no larger fragments have ever been obtained. The deoxyribonucleic acids, on the other hand, are stable to alkali and do not degrade to simple nucleotides. Now the mono-esters of the simple nucleotides are structural analogues of the nucleic acids (in the above scheme of formulae, (III) would represent a nucleic acid if R were a polynucleotide chain). The following scheme (in which Base-$C_{2'}$-$C_{3'}$-$C_{5'}$ represents a ribonucleoside residue) represents the alkaline breakdown of a ribonucleic acid ; it can be seen that it must yield exclusively a mixture of ribonucleoside-2′ and -3′ phosphates, which is indeed the case. The cyclic phosphates shown on the following page as intermediates in this scheme have, in fact, been detected since the time of the original postulate.

The internucleotidic linkage in the above scheme is shown as 3′ : 5′ ; the evidence from alkaline hydrolysis does not of itself distinguish between $C_{2'}$ and $C_{3'}$ as a point of linkage, but, as will be shown later, other evidence allows a decision in favour of $C_{3'}$. The observed stability of deoxyribonucleic acids (which are also formulated as 3′ : 5′-linked polynucleotides) towards alkali, is to be expected ; there being no hydroxyl on $C_{2'}$, they show the normal stability of simple phosphodiesters. It will be convenient



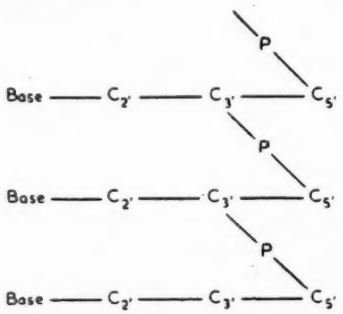
now to summarise the confirmatory evidence for the general structure of the two types of nucleic acid separately.

Deoxyribonucleic acids.—These acids are formulated as linear polynucleotides with a recurring 3′ : 5′-internucleotidic linkage as shown below.



This formulation accords with their stability towards alkali and with the fact that the nucleotides formed by normal enzymic hydrolysis are identical with the synthetic deoxyribonucleoside-5′ phosphates. The production of pyrimidine nucleoside-3′ : 5′ diphosphates on acid hydrolysis is also in accord with this structure and shows the participation of $C_{3'}$ in the internucleotidic linkage. The initial action of acid on deoxyribonucleic acids is to hydrolyse the purine glycosidic linkage yielding apurinic acids. In the second phase these acids are degraded mainly by elimination reactions rather than hydrolysis since each purine-free sugar residue in them bears a phosphate group at $C_{3'}$, i.e., in the β-position to a carbonyl or potential carbonyl group. On this basis pyrimidine nucleoside diphosphates would be expected to arise from those sites in the original acid where pyrimidine residues are flanked by purine nucleoside residues. The alkali-stability of isolated deoxyribonucleic acids would seem to preclude branched-chain structures. The only type of branching which could be envisaged would be on phosphorus and the resulting phosphotriester linkages would be alkali-labile. So far no stepwise degradation method is available for the determination of sequence of residues in deoxyribonucleic acid.

Ribonucleic Acids.—Brown and Todd postulated a recurring 3′ : 5′-internucleotidic linkage for the main chain in ribonucleic acids, i.e. :



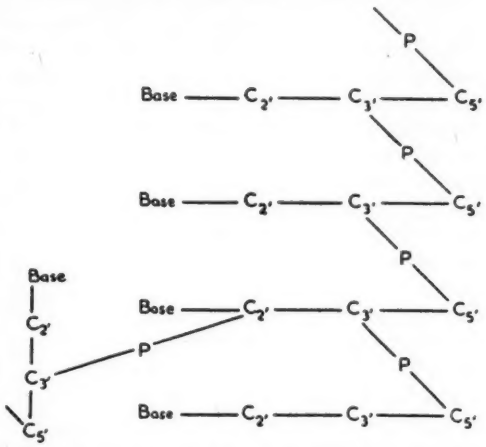
That $C_{2'}$ or $C_{3'}$ is one point of attachment of the internucleotidic link follows, as above indicated, from the results of alkaline hydrolysis. $C_{5'}$ is also involved, since, for example, treatment of ribonucleic acids with snake-venom diesterase or

with ribonuclease followed by intestinal phosphatase, gives large amounts of the ribonucleoside-5′ phosphates. Convincing evidence in support of this structure has come from enzyme studies.

Crystalline pancreatic ribonuclease acts on ribonucleic acid to give a variety of products. Short action yields, in addition to larger fragments, substances which have been identified as the cyclic 2′ : 3′-phosphates of the two pyrimidine nucleosides. Longer treatment yields the pyrimidine nucleoside-3′ phosphates and a variety of small polynucleotides (mainly di- and tri-) in which the terminal residue, bearing a phosphate group at $C_{3'}$, is always a pyrimidine nucleoside residue. This apparent specificity for pyrimidine nucleotides has been confirmed by studying the action of ribonuclease on the cyclic phosphates and monoesters of the mono-nucleotides. Ribonuclease will hydrolyse the cyclic 2′ : 3′-phosphates of the pyrimidine but not the purine nucleotides and it yields exclusively the pyrimidine nucleoside-3′ phosphates. It will also hydrolyse the monoesters of the pyrimidine nucleoside-3′ phosphates to the free nucleotide *via* the cyclic phosphates, but it has no action on esters of the 2′- and 5′-phosphates of the pyrimidine or on those of the 2′-, 3′- or 5′-phosphates of the purine nucleosides. It follows therefore that all the pyrimidine nucleoside residues in ribonucleic acids have the internucleotidic linkage at $C_{3'}$, and analogous studies using spleen nuclease show that all the purine nucleoside residues must be linked at $C_{3'}$. The structural picture of ribonucleic acid given above would thus appear to be well established and there is no evidence for any other type of linkage than that indicated.

The possibility that ribonucleic acids have a branched structure has frequently been canvassed. Bearing in mind the primary requirement of alkali-lability it would appear that only two types of branching need be considered. The first is branching on phosphorus, i.e. through phosphotriester linkages. This has been the type most frequently proposed in the past, but it seems most unlikely. Recent work in Cambridge has shown that the diesters of the nucleoside-2′ and -3′ phosphates (which are structural analogues of branched polynucleotides) are extremely unstable even at approximately neutral *p*H and immediately lose on ester group. It is reasonable to conclude from this that if ribonucleic acids in the cell contained such phosphotriester links, they could hardly survive any normal isolation procedure.

The second type of branching which is theoretically possible is shown below.



Provided that the branch occurs at $C_{2'}$ in one residue in the main chain, and proceeds *via* a phosphodiester link to $C_{3'}$ in the first residue of the branch, the requirements of the hydrolytic evidence are met. There is as yet no satisfactory evidence as to whether such branching does or does not occur in natural ribonucleic acids. It may be that both branched and unbranched structures occur, but although chemical considerations indicate clearly *how* branching could occur, they are silent on whether it does or not, and a decision must await further physical evidence or the progress of synthetic studies in the polynucleotide field.

One of the major differences between individual ribonucleic acids must be the sequence of residues in the polynucleotide chain. A stepwise degradation method for determining sequence has already been proposed. It depends on oxidation of a terminal residue containing a free 2′ : 3′-glycol system with periodate followed by expulsion of the oxidised residue by means of an elimination reaction under conditions which leave the other residues in the chain unaffected. This method has been

applied successfully to small oligonucleotides but its application to the nucleic acids themselves must await the preparation of the latter in a truly homogeneous condition. In theory this stepwise degradation method, if applied to a homogeneous nucleic acid, would also serve to discover if chain-branching occurs.

It is of some interest to consider how the nucleic acids are built up in nature. There could be, in theory, at least two types of polynucleotide, the first bearing the terminal phosphate at $C_{3'}$ in the nucleoside residue and the second with the terminal phosphate at $C_{5'}$. The 'monomers' for the first type (Type I) would be the nucleoside-3′ phosphates and for the second (Type II) the nucleoside-5′ phosphates. End-group assays on isolated ribonucleic acids have shown examples of both types, but this is hardly decisive evidence since any degradation of a Type II acid would inevitably yield materials with end groups corresponding to Type I. The fact that the nucleoside-5′ phosphates and polyphosphates are of common occurrence in nature, coupled with the known capacity of polyphosphates to act as phosphorylating agents, seems to me to suggest that the normal nucleic acids are likely to be polyesters in which nucleoside-5′ phosphates are the 'monomeric' components. It is true that the action of ribonuclease is reversible, and it has been shown that it can be used to prepare small oligonucleotides from cyclic nucleoside-2′ : 3′ phosphates, but I think it unlikely that this is the normal synthetic mechanism although it may play a part in abnormal conditions. It is of great interest in this connection that Ochoa has recently described an enzyme system which rapidly makes polynucleotides from naturally occurring nucleoside-5′ pyrophosphates (e.g., from ADP and UDP). This discovery of a method of enzymic synthesis, together with the experimental demonstration that unnatural heterocyclic bases can be assimilated by organisms and built into polynucleotide chains, would seem perhaps to open up important vistas in the field of chemitherapy of malignant disease as well as in virus studies.

Step-wise synthesis of polynucleotides which in the nucleic acid field is analogous to polypeptide synthesis in protein chemistry is still in its infancy. Recently, Michelson and Todd have reported the unambiguous synthesis of a dithymidine dinucleotide containing the 3′ : 5′-internucleotidic linkage, and have shown its behaviour towards enzymes to parallel exactly that of the dinucleotidic fragments found in deoxyribonucleic acid hydrolysates. Extension of these studies is desirable for, although it is true that no small natural polynucleotides have been reported analogous to the natural small polypeptides, such molecules might have interesting biological properties, and a study of them might throw some further light on details of nucleic acid chemistry. An obvious, although elusive goal, too, is the discovery of a method by which long polynucleotide chains could be built up easily in the laboratory.

These possibilities of synthesis become the more attractive when we consider what is known about the functions of nucleic acids, since these functions may well depend to a substantial extent on the conformation of the macromolecules of the nucleic acids. This problem of macromolecular conformation has received much attention in recent years, and, indeed, a great deal of publicity, so that a few words about it are desirable. Most of the X-ray studies on which it rests have been done on salts of deoxyribonucleic acids which can be obtained in crystalline fibre form and strictly speaking the physical picture of nucleic acid which is widely current—that of the double helix—applies only to deoxyribonucleic acids. The ribonucleic acids are much less tractable and do not easily give fibres which can be studied, so that knowledge of them is still scanty.

Watson and Crick have developed a model of the deoxyribonucleic acids which accommodates the chemical evidence of structure discussed above, and agrees in essentials with the elegant X-ray evidence of Wilkins, Franklin, Gosling and others. According to this model the molecule consists of two polynucleotide chains in the form of right-handed helices, coiled round the same axis and held together by hydrogen bonds to form a double helix. In this model the phosphate groups are on the outside of the helix and the purine and pyrimidine bases, which lie inside

the helix at right angles to its long axis, are held together by hydrogen bonding to form a stable system. A novel feature is the way in which the two chains are held together by the purine and pyrimidine bases which are bonded in specific pairs. Based on molecular models, it is believed that only the pairs adenine-thymine and guanine-cytosine can bond together and at the same time fit into the structure. One of the powerful arguments advanced in support of this model is that analysis of a variety of deoxyribonucleic acids shows that the ratios adenine/thymine and guanine/cytosine are approximately unity, whereas the ratios between other pairs such as adenine/guanine vary widely from acid to acid. There would seem to be little doubt from recent X-ray work by the workers in King's College, London, that the deoxyribonucleic acids exist as two coaxial helical chains related by a dyad axis as postulated by Watson and Crick, and it appears that the third coaxial chain, which is absent in the nucleic acid, is formed by the protein component in the nucleoproteins. The further implications of the Watson-Crick model, although speculative, are none the less interesting.

Consideration of the specific hydrogen-bonding between pairs of bases shows that the two strands of double helix represent identical polynucleotide chains which, however, run in opposite directions. If we imagine the two strands separated from one another and in a medium where polynucleotide synthesis is in progress, then we might expect that because of the specific pairings of the bases, each strand would build up on itself a second strand so constituted as to duplicate the original double helix. In other words each strand would be, in fact, a permanent template which would ensure that the deoxyribonucleic acid synthesised would always have precisely the same sequence or pattern. This might give an important clue to the way in which hereditary patterns are handed on when organisms reproduce—the deoxyribonucleic acids passed from the mother cells would be permanent templates for the daughter cells. There are, of course, some difficulties about this simple picture —notably the difficulty of unwinding the chains in a very long double helix if the molecular weight of the acids is several millions. Various suggestions have been made which might make this difficulty less real, but it is also worth remembering that estimates of the molecular weight of the nucleic acids are variable and perhaps not too reliable.

Now the picture thus sketched of the part which could be played by deoxyribonucleic acids in heredity is grossly over simplified, but that they do play a part is certain. Deoxyribonucleic acid is a characteristic constituent of the chromosomes and all the cellular deoxyribonucleic acid is probably associated with them. The chromosomes are, of course, the carriers of the Mendelian genes, and a number of observations, notably in the field of bacteriological chemistry, suggest that deoxyribonucleic acid may indeed be the genic material. Thus, it was shown first by Avery in 1944 that deoxyribonucleic acid extracted from a ' smooth ' strain of pneumococcus would transform a ' rough ' strain of this organism into a ' smooth ' strain, and similar ' transforming factors '—all deoxyribonucleic acids—have since been found by other workers in a variety of bacteria. It has been established by extended experiments that these factors have many of the properties of genes, and it seems indeed that some deoxyribonucleic acids may themselves be the genes of bacteria. The vistas opened up by these findings are fascinating, but I shall not discuss them further here since they lie, at least for the present, more within the province of the geneticist than the chemist.

Much circumstantial evidence has been adduced for the view that ribonucleic acids are intimately concerned with the synthesis of proteins in living organisms, and it seems that this is probably one of their main functions. There is much less evidence for the direct participation of deoxyribonucleic acids in protein synthesis. This has not prevented much speculation, largely mathematical, based on the Watson-Crick model, regarding the means by which a macromolecule built up on this pattern of, say, four individual nucleotide components, can provide the necessary ' information ' which will permit the building up of a specific sequence of amino-acids in the polypeptide chain of the protein, there being more

than twenty amino-acids to choose from. It must be admitted, of course, that, since such evidence as is available suggests that the ribonucleic acids too have some kind of helical structure, probably rather like deoxyribonucleic acids, the exercise may not be without significance since much the same problems will be presented in either case. In the Watson-Crick model, or indeed in any other model fitting the chemical facts, the phosphorus atoms in a nucleic acid are spaced at a distance from one another not greatly dissimilar from that between the peptide linkages in a polypeptide chain. Under these circumstances our knowledge of the chemistry of the phosphoric acids and of their mixed anhydrides as studied in the laboratory or in enzyme reactions, enables us to picture a mechanism by which polypeptide chains could possibly be synthesised on a nucleic acid chain which would act as a kind of template. For, if the free acidic group of each phosphate linkage in the polynucleotide condensed with the carboxyl of an amino-acid to form a mixed anhydride, then under appropriate conditions of base catalysis one would expect that the anhydride links would rupture and the amino-acids consequently link up into a polypeptide. This picture does not, of itself, present a solution of the major problem—how do we account for the production of a specific sequence of amino-acid residues? The picture is very complex—not least because we are not certain how specific the sequence of residues is in many proteins—but it is my belief that an attempt to provide an answer by mathematics alone, without bringing into full consideration the chemistry of the process by which the polypeptide is synthesised, is not likely to be successful.

We are thus at an exciting stage in the study of the nucleic acids. We have now a substantial knowledge of their chemistry and their physical form, and we have a good deal of information available about their biological significance and possible function. In the popular mind the proteins are often thought to be the central substances in living matter. I hope I may have conveyed to you the opinion of the workers in the field I have discussed, that the nucleic acids must rank as at least their equal and that further study of these fascinating substances and their behaviour jointly by the chemist, biochemist, biophysicist and biologist may open new chapters in our knowledge of life and heredity and in the conquest of disease.

THE GEOGRAPHICAL DISTRIBUTION OF PAST FLORAS

Address by

W. N. EDWARDS

PRESIDENT OF SECTION C

O God ! that one might read the book of fate,
And see the revolution of the times
Make mountains level, and the continent,
Weary of solid firmness, melt itself
Into the sea! and, other times, to see
The beachy girdle of the ocean
Too wide for Neptune's hips. . . .
Henry IV, Pt. 2, Act III, Sc. 1

An American botanist, Theodore Just, might have been echoing King Henry the Fourth's apostrophe when he said that 'if the distribution and extent of land masses and oceans throughout geological history were known in greater detail, problems of plant distribution would offer fewer difficulties.' In one sense this statement is a truism, in another it is a salutary warning to plant geographers to look at their problems from an historical point of view. The revolutions of past times, the encroachments and retreats of seas, the sinking and the rising of lands, the wrinkling of the strata into mountain ranges and their subsequent erosion, are reflected in the altered distribution of faunas and floras over the world. Past climatic changes, themselves in some measure at least due to geological causes, influence the distribution or even the existence of plants and animals, and can be inferred with varying degrees of accuracy from the nature of contemporary sediments and of their contained fossils. The interactions of these and other factors, cosmic and terrestrial, organic and ecological, await the interpretation of the biogeographer, who should remember that even if 'all time is eternally present' he must look closely at more than the momentary flash of to-day.

Sir Edward Bullard said recently that, to deal adequately with the wider problems of geology, one should in addition to being a geologist, be a chemist, a physicist, an astronomer, a cosmologist, an oceanographer, a meteorologist, a palaeontologist and a mathematician. He ought, of course, to have added a botanist and a zoologist. Since no one possessed such qualifications, Sir Edward went on, everyone who discussed these problems was talking to some extent about things he did not really understand. However, considerations of this sort do not, I am glad to say, deter any of us from talking about obscure problems. That is, indeed, one of the methods by which scientific knowledge progresses, and we talk about things we do not fully understand partly in order to clarify our own thoughts—though we sometimes unaccountably fail to clarify the minds of other people.

The study of palaeontology has many aspects, stratigraphical, morphological, evolutionary, and so forth. For some, palaeontology is a mere branch of the nobler science of statistics ; for others, concerned with technical applications, it is a variant of philately in which, on the microfossils which take the place of postage stamps, one may ignore the postmark of their organic origin. The biogeographer of the past draws maps, and then other palaeontologists tell him that his maps are not worth the paper they are printed on. Perhaps one should call them diagrammatic approximations based on imperfect data.

A few years ago the Geological Department of the British Museum arranged a series of exhibits summarising the succession of life through geological time, and

illustrated each period with a palaeogeographical map. Subsequent requests for reproductions of these maps confirmed our opinion that they might be of use to students and others, and the Trustees of the British Museum have agreed to their publication in booklet form, as a companion to the handbook describing the exhibit as a whole. In spite of the strictures of the perfectionist, such maps may at least be a stimulus to further research ; like any other tool, they should be used with caution.

Palaeogeographic plans of restricted areas are less unsatisfactory than those which attempt to take in the whole world, which, after all, applies to some extent even to maps of present-day geography, since the surface of the globe cannot be represented on a flat plane without distortion. The biogeographer should, therefore, make use of different map projections to suit the various problems which he is considering ; to take an obvious example, circumpolar distributions should never be plotted on Mercator's projection. In this connexion I would draw attention to the report of a symposium on *The Cartographical Presentation of Biological Distributions* arranged by the Systematics Association and the Royal Geographical Society and issued by the latter as a separate publication in 1954 ; particularly the contribution by the late Margaret S. Anderson.

Maps which attempt to portray the past distribution either of land and sea, or of faunas and floras, or of climates, offer one means of attempting to co-ordinate the ever-increasing accumulation of facts in palaeontology, and even of looking at long-known facts from a different angle. Those who have been interested in the geographical distribution of fossil plants have often tended to lay emphasis on the altered distribution of individual genera, such as *Ginkgo* or *Liriodendron*, or of wider systematic groups, such as ferns or araucarian conifers, rather than of plant formations or types of vegetation. The ecological approach, which has perhaps received more attention among American and Russian palaeobotanists, is likely to give a better picture of vegetational development in relation to climatic and geographical changes than the systematic ; it is also valuable because a scanty and fragmentary assemblage of fossil plant remains may yield information about the kind of vegetation represented, and hence of the climate, even if the specimens cannot be identified with certainty. One can, for example, following Sinnott and Bailey and others, recognise a cool-temperate dicotyledonous forest flora by the high proportion of leaves with toothed margins, whereas a high proportion of entire-margined leaves would indicate a tropical flora.

In offering some selected observations on plant distribution in the past, one may, for the brief hour of a presidential address, concentrate on a few main questions : during what periods, if any, were the land floras more or less uniform over the globe ? To what extent did they become diversified ? What is the relation of these changes to geological and climatic conditions, and how does the plant evidence fit in with various theories of crustal change ? What bearing has the phytogeography of the past on the evolution and dispersal of major groups like the flowering plants ?

I propose to begin, omitting the earliest land floras, with the Upper Devonian. At the very end of this period and in the lowest Carboniferous there was an extremely widespread flora, the significance of which has recently been emphasised by W. J. Jongmans. This transition flora is particularly characterised by a lycopod tree called *Lepidodendropsis*, a genus which was first distinguished from the well-known *Lepidodendron* some twenty years ago. Indeed, until recently most specimens of *Lepidodendropsis* from lower Mississipian or perhaps uppermost Devonian beds have been referred to as *Lepidodendron*. Jongmans, however, now concludes that no true *Lepidodendron* is known from this horizon. Floristically, the top of the upper Devonian is perhaps marked by the disappearance of the fern-like *Archaeopteris*, which rarely if ever extends into the transition flora ; its place in the latter is taken by *Rhacopteris* and *Triphyllopteris*. The lycopod *Cyclostigma*, on the other hand, characterises upper Devonian floras with *Archaeopteris* and is also an associate of *Lepidodendropsis*. So far no regional differences have been observed in this widely distributed flora, which is known from South America, the United States, Spitsbergen, Europe, north

Africa, south central Asia, China and Australia. It has not yet been recorded from the southern half of Africa.

As we pass upwards through Carboniferous strata, the flora shows more and more regional diversification. By the close of the period there are four main types of vegetation, the Euramerian (the familiar Coal Measure flora of Europe and eastern America), the Angaran in northern Asia, the Cathaysian or Gigantopteris flora, and the Gondwana or Glossopteris flora. In addition, in the western United States, there are floras differing from that of the Euramerian Coal Measures, and sometimes showing affinity with those of eastern Asia. Furthermore, within the Euramerian area different plant associations can be recognised, for the paralic basins (those of the coastal plains) have on the whole a richer flora than the limnic inland basins. Jongmans compares these limnic basins to islands which tend to develop endemic species.

The relation of the vegetational development thus briefly outlined to the physical state of the earth is fairly clear. The close of the Devonian was a time of marine regression : thus, a maximal land emergence gave the land flora its greatest opportunities for spreading. The uniformity in composition of this wide-spread Lower Carboniferous flora in turn suggests a much greater climatic uniformity than we experience at the present day : the arctic was more temperate, and perhaps the tropics were less tropical. Then world conditions began to change : a period of mountain building and of marine transgressions and fluctuations was accompanied by the approach of an ice age. Possibilities of plant migration lessened ; isolation and differentiation of floras followed. At their lines of contact, floras generally quite distinct might well show a mixture of types here and there. Such mingling, if fully substantiated, might be of great value to the biogeographer in suggesting connexions and migration routes, but one must beware of building on imperfect identifications. Two supposed cases of the mixture of northern and Gondwana plants may be briefly mentioned. The first refers to records of northern species of lepidodendroids in the Gondwana flora ; the second to records of glossopterids in Russia. I have elsewhere tried to show that '*Sigillaria brardi*' and various presumed species of *Lepidodendron* and *Lepidophloios* from Brazil and South Africa should not, in fact, be referred to these genera at all, and since no true *Lepidodendron* has so far been found in the early Mississipian it seems probable that the northern and the southern lepidophytes evolved divergently from members of the *Lepidodendropsis* group.

The supposed *Glossopteris* from Russia, originally produced by Amalitzky, who was not a botanist, as a tit-bit for the participants in the Geological Congress of 1897, was not even figured until many years later, but found its way into textbooks with astonishing rapidity. When the Geological Congress again went to Russia, in 1937, several visiting palaeobotanists were able at last to see the actual specimens, which turned out to be completely worthless as records of *Glossopteris* ; the Russians themselves were meanwhile putting these and other somewhat similar leaves into new genera. One still sees these leaf genera referred to as closely related to *Glossopteris*, but there does not seem to be a shred of evidence for this view; our knowledge of *Glossopteris* is just beginning to expand, for abundant fructifications have turned up in South Africa and in Brazil, and records of northern glossopterids should now be treated with the utmost suspicion unless they are based on the very characteristic fructifications.

To return to the Upper Carboniferous and Permian floral provinces. The plant succession in Morocco and Algeria is of the Euramerian type ; that is to say, the Euramerian flora was established on both sides of the Carboniferous Tethys. What then if not the Tethys sea was the dividing line between the northern and southern provinces ? A distributional map suggests a comparative absence of fossil plants along a belt stretching across Central Asia towards Arabia and then over Africa, in exactly the same position as the desert belt of to-day and the desert belt of the Mesozoic, which will be discussed shortly. There is lithological evidence of aridity in the Permian of Morocco, and across the rest of North Africa the post-Carboniferous variegated beds of the unfossiliferous Nubian Series may well date

from this period. It seems feasible that the *Glossopteris* flora was bounded partly by an arid belt, partly by seas. And possibly where it abutted on Cathaysia it met an almost impenetrable barrier in the richer closed formations of a more humid and equable climate, though here in the present Indonesian area one might expect to find, if anywhere, an admixture.

The evidence for climatic zonation is clear. The east-west Euramerian belt with abundant and varied vegetation, lacking annual growth-rings in the wood of its trees, bespeaks a warm, wet climate without marked seasons. Jongmans, following a very general opinion at the present day, thinks that this climate was not tropical, partly because he considers a tropical climate to be inimical to coal formation and partly because the flora was marginally mixed with more temperate elements. However, tropical and sub-tropical floras usually shade off into warm-temperate, and it is by no means certain that peat could not accumulate in very humid tropical regions and be eventually transformed into coal. Tropical forests are not necessarily so hot as some might imagine ; Reid & Chandler quote figures giving an average temperature in a tropical African rain-forest of 68° F., with a maximum daily range of 12° F., the average temperature at a station away from the forest being much higher, with a daily range of 30° F. The forests encouraged by a humid and equable climate would themselves tend to lower the temperature, and thus the average temperature of tropical regions in such periods might well be much lower than at the present day. One need not, therefore, quarrel with Kryshtofovich's view that the Euramerian flora with its Cathaysian continuation might be regarded as essentially of a tropical character. (Since we ourselves are living in a glacial period, we tend to think that the present wide extension of the cool-temperate frosty zone is ' normal ' whereas, in fact, it is abnormal in world history.) North and south respectively of the Euramerian belt are the temperate Angara and Gondwana floras, with wood showing annual rings as one of the signs of seasonal growth conditions. Indeed, the superficial resemblances between some Angara and some Gondwana plants may be due to similar climatic and other ecological factors rather than to close systematic relationship.

The main apparent general differences between these two temperate floras is that the Angara flora is more varied (even allowing for Zalessky's unnecessary multiplication of names). The plant evidence does not stand alone in this respect, for Martynov claims that the insect fauna of the northern zone is also richer. This may be because the Gondwana region, with its dispersed centres of glaciation, was in general cooler, but may also be a consequence of southern aridity. Gondwana coal basins seem to represent lacustrine areas in a dry climate.

Jongmans and others put the floral differentiation of the Carboniferous in the Stephanian. It was certainly well established by then, but equally certainly started earlier, and though Jongmans has maintained ' that in the Westphalian the flora was entirely uniform all over the world,' this extreme view requires much more detailed evidence before it can be accepted. The familiar Westphalian Coal-Measure flora of the Euramerian province is either absent from the southern hemisphere or is poorly developed there; apart from the survival of a few intrusions and marginal mixtures, including the petrified ferns, *Psaronius* in Brazil and *Tubicaulis* in Tanganyika, it is dubiously represented by a few uncertain types.

This mid-Carboniferous separation, with its consequent divergent floral evolution, left its mark on the land vegetation for a very long time ; even, I think, to the present day. The earlier Mesozoic floras of the northern and southern hemispheres remained distinct. The Upper Triassic *Dicroidium*-flora of the southern hemisphere is as different from the contemporary northern flora as was the *Glossopteris* flora which preceded it. I regretfully pass over even a superficial consideration of these vegetational provinces in order to reach the middle Jurassic, by which time world vegetation gives the appearance of having again approached uniformity. But this uniformity is not so complete as some have supposed, and may be illusory. Florin's analysis demonstrates convincingly that in Mesozoic times the southern hemisphere conifers differed widely from those of the

north. Thus, in spite of the superficial resemblance between certain conifer impressions from Graham Land in Antarctica and from Yorkshire, they belong not only to different genera but to different families (Podocarpaceae in the south, Taxaceae in the north). Florin traces the divergent evolution of the conifers from the Upper Palaeozoic onwards, and points out that such traffic as there was between the two hemispheres was mainly via the Indonesian-Australian region. Indeed, this region, which includes the south-eastern part of the old Cathaysian province, seems likely to have been the main centre of evolution from which most of the southern hemisphere conifers radiated in the early Mesozoic. They spread mainly southwards because there they had more room for expansion. In the northern hemisphere the appropriate ecological niches were occupied by a varied group of conifers which had already proliferated before the end of the Palaeozoic. The failure of most truly northern conifers to spread southwards may have been due again to the equatorial belt of dense closed forest as well as to seas and deserts.

Other groups of Jurassic plants have not yet been analysed so carefully as the conifers, but both Halle and Seward, who were among the leading champions of Jurassic floral uniformity, admitted some regional peculiarities and noted, for example, that several groups (including the Ginkgoales) were absent from the Graham Land flora. A point worth emphasising is that the wide distribution of a Jurassic fern such as *Coniopteris* no more supports cosmopolitan uniformity than does the equally wide distribution of the bracken fern (*Pteridium*) at the present day.

In addition to the evidence for mid-Jurassic differentiation between the northern and southern hemispheres there are also indications of latitudinal zonation. Kryshtofovich has pointed out that in the great eurasiatic land mass there was a northern region where cycadophytes were absent or rare, and a more southerly belt where they were abundant. In the northern region flourished the Siberian middle Jurassic flora described long ago by Heer and others, characterised particularly by the abundance of *Ginkgo* and allied genera and by the paucity of cycadophytes and conifers. The Jurassic cycadophytic belt roughly coincides with the region long previously occupied by the upper Coal Measure flora. Lepidophytes and pteridosperms had been replaced by conifers and cycadophytes, but the floral belts had remained in much the same position.

In the old world two geographical features played an important part in vegetational development. One was the Tethys sea, and the other the arid belt running across central Asia into north Africa. These two cross each other obliquely, but whereas the Tethys had a continuous existence from the Palaeozoic to the Tertiary, the land belt was not continuously and invariably dry; at times a desert zone, it was at other times more humid. The middle Jurassic was one such humid period; hence, the wide diffusion of land plants which together with the near-cosmopolitan distribution of a few types gives some appearance of uniformity to the flora. Desert conditions, more pronounced in some parts of the arid belt earlier in the Triassic, set in again towards the close of the Jurassic. In the latest Jurassic and early Cretaceous deposits of this belt there are red beds and deposits of gypsum and potash, with plant fossils rare or absent from Mongolia and Central Asia to Kazakhstan and Persia. Desert conditions continued across Arabia and north Africa. In adjoining regions humidity decreased and by Wealden times the rich Jurassic flora was restricted to a few lacustrine or riverine areas, or to places with a high local rainfall. One of the most characteristic and widespread Wealden plants is the problematical *Weichselia*, which though it has not yet revealed to us its systematic affinity has long been recognised as a strand or dune plant of pronounced xerophytic character. At many Lower Cretaceous localities in Europe, Africa and western Asia *Weichselia* is either the only plant to be found or is associated with other xerophytes such as the conifer *Frenelopsis*. The most characteristic Wealden ferns include those related to the living genera *Dipteris*, *Matonia* and *Gleichenia*. The first two of these survive only in the Malayan region, and only in places fully exposed to the sun; they are not shade ferns and cannot live in dense forest. Many species of the purely tropical

Gleichenia are also ferns of the open with somewhat xerophytic fronds. The widely distributed *Gleichenia* was indeed ecologically the bracken (*Pteridium*) of the Lower Cretaceous.

The Mesozoic forests were clearly thinning out under the influence of increasing aridity, which must have been largely due to that uplift and land emergence connected with the neo-Cimmerian mountain-building movements. The consequent enlargement of land areas naturally also favoured the dispersal of Lower Cretaceous plants. By Aptian times a new element had appeared in the flora : the angiosperms or higher flowering plants. In the Lower Greensand of southern England five different types of dicotyledonous trees are represented by their well-preserved petrified wood. One of these, *Woburnia*, is most probably referable to the Dipterocarpaceae, now confined to tropical Africa and the Indo-Malayan region ; another, *Aptiana*, may belong to the Theaceae, a tropical and sub-tropical family. The affinities of the other three are still uncertain, but as far as their wood anatomy is concerned Dr. M. C. Stopes, who first described them, concluded that not one of the five showed any primitive features ; all were highly developed dicotyledonous types. They are still, after forty years, among the oldest satisfactory angiosperm fossils. Of numerous vague and dubious records of pre-Cretaceous angiosperms I will here dispose of only one, since it concerns a wood with rather primitive structure, at one time supposedly referable to the dicotyledons ; Sahni's Lower or Middle Jurassic *Homoxylon* is unquestionably, as Sahni himself suspected, the wood of a cycadophyte. M. N. Bose's investigation of a bennettitalean stem from the Jurassic of the Rajmahal Hills has shown that its secondary wood was identical with that of *Homoxylon*, which should no longer be cited as a Jurassic angiosperm. All palaeobotanists would agree that angiosperms must have been in existence before the end of the Jurassic, but let us be certain of them before we indulge in flag-waving.

The Lower Cretaceous flora of Cercal in Portugal, long regarded as containing the oldest known impressions of dicotyledonous leaves, has recently been revised by Texeira, who thinks that it is not quite so old as was formerly claimed ; he refers to it in one place as perhaps Aptian, and in another as at the junction of the Aptian and Albian. That is to say, about the same age as the English Lower Greensand dicotyledons or a little younger. Of the three or four species to which Texeira reduces Saporta's dicotyledons from this locality, two may perhaps pass muster. There are also records, at present I think not fully confirmed, of Aptian dicotyledons in eastern Transcaucasia and in the Moscow basin.

Certainly by Albian times there were in many widely separated localities plant associations in which numerous angiosperms were mixed with conifers, ginkgos, cycadophytes, ferns, and sometimes the lingering *Weichselia*. Such mixed Albian floras are found in the Potomac beds of Maryland, in Portugal, in western Kazakhstan, in the Nikan formation in Ussuriland, and in the Zyrianka formation of the Kolyma basin. In southern England the few plant remains in the marine Albian include conifers and rare fragments of *Weichselia* together with petrified dicotyledonous woods which have not yet been investigated.

Thus, angiosperms, belonging incidentally to a considerable range of families, had become widely distributed during the later parts of the Lower Cretaceous, though they were not yet dominant. Their final victorious expansion must have been mainly due to the Cenomanian transgression ; certainly in the Tethyan region. In mid-Cretaceous times the dwindling relics of the Mesozoic cycadophytic vegetation, caught between the Devil and the deep sea—between the desert belt and Tethys—were finally extinguished. A few of the older forms survived here and there, of course ; *Dipteris* and *Matonia* still live in the Malayan region, for example, but such survivals are likely to be as a rule on the fringes of their former areas of distribution. Plants are often said to migrate, or even to be driven out, as a result of climatic changes, but for plants extinction in adversity is always more probable than migration ; they are more likely to spread in stable and favourable conditions. This is, perhaps, an appropriate opportunity to mention Willis's Age and Area hypothesis. The main idea behind it, that the

larger the area occupied by an organism at the present day the older it must be, is directly at variance with much in the palaeobotanical record. However, a number of phytogeographers saw fit ' to sport with J. C. Willis in the shade,' though probably most of them have now extricated themselves from the statistical tangles of Age and Area.

Later in the Cretaceous as the seas receded the angiosperms, ready at hand, simply colonised the vacant spaces. During the Cenomanian they were flourishing and have left their remains in Portugal, in France, in Bohemia, in Egypt, in various parts of the U.S.S.R., in North America, and perhaps in Greenland. Incidentally most of these Cenomanian and earlier floras with dicotyledons have a distinctly xerophilous habit indicating a continuation of the arid climate.

Darwin once wrote, in a much-quoted passage, that ' the rapid development as far as we can judge of all the higher plants within recent geological times is an abominable mystery.' But those words were written in 1879, when no Lower Cretaceous angiosperms had been found, most of the important Cenomanian floras had not been worked out, and little was known even of the rich dicotyledonous floras of the late Cretaceous. The accumulating mass of evidence suggests that we may soon have to reverse Darwin's wonderment and enquire how it was that the angiosperms took so long to spread and multiply.

The question arises whether the floral succession and the geographical conditions throw any light on the period or the region in which the angiosperms may have originated. A number of well-differentiated and fairly widely distributed angiosperms are known from the Aptian; records from the lowest Cretaceous beds are at present doubtful, and I know of no really acceptable angiosperm fossils from the Jurassic. Clearly, however, the group must be older than the Cretaceous. In reviewing the past history of the insects, F. M. Carpenter remarked recently that although in the Jurassic there were many insects belonging to living families, there were ' none of the higher Diptera, many of which are now conspicuously associated with flowering plants ' and that bees and wasps were also absent. This does not, however, take us very much further, and gives no indication of when the mutual adaptation of the higher flowering plants and the higher insects may have started, important as this factor may have been at a later period in assisting their rise and proliferation.

Kryshtofovich and also Vakhramiev have suggested definitely that the angiosperms originated in the Jurassic. It is pointed out, and quite rightly, that normally one might expect new forms in a flora to be revealed only after they have attained to some dominance, and not when they first arise and play a very minor part. Kryshtofovich then argues that the fundamental morphological evolution which produces new groups of plants goes on over a period of many million years during stable geological conditions—and the Jurassic was such a stable period. A sharp physico-geographical change leads to a selection of forms better adapted to the new conditions, and they rapidly spread into the spaces left free by the disappearance of other forms. The latter part of the argument is perfectly acceptable, but that major evolutionary changes mature slowly in stable periods seems to me inherently unlikely. Of course, the term ' stable period ' is an imperfect generalisation: conditions were never universally static. Minor and local physico-geographical changes must have been constantly moulding living communities and promoting diversification within groups of organisms already in existence. But the view that major evolutionary outbursts were associated with major geological events is at least as probable as Kryshtofovich's. A cycle of uplift and mountain building, with its extension of land surface implying, firstly, easier land connexions and, secondly, the development of arid regions, undoubtedly facilitates the spread of some land organisms, and the extinction of others. Can one, however, go further and suggest, for example, that the physiological stimulus of increasing aridity has a close connexion with the origin of new groups? Whatever the answer, the importance of desert belts in relation to vegetational evolution deserves to be stressed. The late-Jurassic-early-Cretaceous aridity clearly played its

part in the rise of the flowering plants, but was too late to be considered responsible for their origin. There is often a very long interval between the first appearance of a group and its extensive proliferation. Mammals are known from the Triassic, but only became important towards the end of the Cretaceous ; the main efflorescence of the ostracoderms began at the end of the Silurian, but they have been recorded from the middle Ordovician of Colorado. Conifers and ginkgophytes, prominent in the Mesozoic, are known from the Carboniferous. The angiospermic production of seeds adapted to survive drought has been claimed as one factor leading to the success of the higher flowering plants, but seed formation goes back to the Devonian, and several distinct groups of Carboniferous plants had evolved the seed. Without touching on the more purely botanical aspect of possible angiosperm ancestors one may merely suggest that these ancestors took the angiospermic turning well before the end of the Jurassic, perhaps even as a result of the geographical and climatic turmoils at the close of the Palaeozoic.

Kryshtofovich, like Seward, favoured an Arctic origin for the angiosperms ; this idea is partly based on the supposed early Cretaceous age of the Greenland Kome flora. But the general opinion now is that the Kome flora is not so old as was formerly thought and may even be later than Cenomanian. Vakhramiev has argued that northern Asia was in fact a refuge for some of the last remnants of the Jurassic flora, and that angiospermic vegetation only reached the Arctic regions in the Upper Cretaceous. One may think that the Arctic or Antarctic regions (if they have always been where they now are) with their long winters of comparative darkness are most unlikely places for the development of new groups of land plants, but it is at present impossible to suggest with any degree of probability where the angiosperms may have originated.

Long before the close of the Cretaceous, then, the vegetation had become predominantly angiospermic over the whole world. This, however, does not imply vegetational uniformity. Returning to the Tethyan region, one finds that Upper Cretaceous beds at various places both north and south of the Tethys sea have yielded remains of dicotyledons related to tropical genera of the present day. In England tropical plants are known from the Lower London Tertiaries, and the tropical Ypresian flora of the London Clay has long been famous. The case for tropical floras in the region of southern England in middle Eocene times (Ypresian and Lutetian) is overwhelming, and the corollary that the climate may also be described as tropical is inescapable. In their discussion of the London Clay flora and the conditions under which it flourished, Reid & Chandler tacitly assume that the ' tropical ' climate was exceptional ; they argue that it came about through extensions of the Tethys sea bringing warm currents from the Indo-Malayan region and do not discuss the climate or palaeogeography of any pre-Eocene period. But the Tethys sea had been in continuous existence, whatever the fluctuations of its outline, for some two hundred million years ; that is, from Carboniferous times. The argument, which seems perfectly feasible, that a milder climate was due at least in part to an east-west sea connected with equatorial waters, must also apply to earlier periods of geological history. In my view this milder climate in our latitudes, supporting, if you like to call them so, tropical or subtropical floras, was up to the Oligocene period the normal condition. At times the climate was very humid, at times drier, and the Eocene was a humid period. For the London Clay, Reid & Chandler have admirably summarised the probable situation : ' Given suitable conditions of precipitation, of the balance between precipitation and evaporation, of the seasonal distribution of the rainfall, of a very equable climate, and a frostless winter, the London Clay flora might have lived under a mean annual temperature of about 70° F.'

The botanical affinities of this flora are very closely with that of the tropical Indo-Malayan flora of the present day. (Incidentally, if we can call the London Clay flora tropical, we might surely apply the same epithet to the Carboniferous Euramerian flora.) Reid & Chandler considered that Indo-Malayan plants, spread along the Tethyan belt into western

Europe in the Eocene. I should here like to emphasise the immense importance of the Tethys sea as a migration route for plants. Running roughly east and west any viable parts of plants carried by marine currents would land up in much the same climate even if they travelled some thousands of miles. The beans of the tropical climber *Entada* have actually been germinated after taking perhaps a year on their journey from the West Indies to the Hebrides or Iceland. Had they journeyed along the Tethys sea—as possibly they did once—they would surely have been able to establish themselves in hospitable localities. Throughout its long history the Tethys, both by marine currents and by migration along its maritime fringes, must have been an extremely important agent in the latitudinal spread of land plants. We have already seen that the Upper Carboniferous Euramerian flora established itself south of the Tethys as well as north, and so did the Mesozoic flora. The same is true of the dicotyledonous vegetation of the late Cretaceous. A very important florule from the Danian of Egypt has recently been described by Miss M. E. J. Chandler, and its elements (including *Palaeowetherellia* and *Lagenoidea*) are either identical with or closely related to those of the London Clay, and, therefore, are comparable with the living flora of the Indo-Malayan region. A few Maestrichtian plants of similar affinity (including members of the Anonaceae and Icacinaceae, families also represented in Egypt) have been recorded from Nigeria and Sierra Leone by Miss K. I. M. Chesters.

The ' London Clay ' type of flora with Indo-Malayan affinity has sometimes been called the Tethyan, but in view of the very different Palaeozoic and Mesozoic floras of the Tethyan belt it is preferable to use Kryshtofovich's term *Poltavian* for the evergreen tropical or sub-tropical flora. Kryshtofovich long ago recognised that in early Tertiary times the vegetation of the Old World was zonally arranged from the tropics to the Arctic regions, and he took the term Poltavian from the Poltava Sandstone in the Ukraine, where the Eocene flora contains *Nipa* and *Sabal*. North of this evergreen vegetational belt was a temperate province of deciduous forests which Kryshtofovich called the *Turgaian*, after the name of a region from which Heer first described an early Tertiary flora nearly a century ago.

Reid & Chandler assumed that the Poltavian flora of the London Clay had migrated from the Indo-Malayan region during the Eocene ; in her recent paper on Upper Cretaceous and Eocene plants from Egypt Miss Chandler further discusses distributional problems and, rightly dismissing theories of the Arctic origin of Poltavian plants, concludes that the early and mid-Cretaceous angiosperm flora, uniform in the tropics, was exterminated in the Tethyan region by the Cenomanian transgression. She then argues that the Poltavian flora re-colonised western Europe in post-Cretaceous times direct from Indo-Malaya. As the climate became more humid in the early Tertiary, far eastern equatorial migrants would no doubt have improved opportunities of establishing themselves and no doubt also there were changes in the composition of the flora. There seems, however, no reason for assuming that Poltavian elements were not continuously invading the western Tethyan regions, both north and south of the sea ; there are Middle and Upper Cretaceous plant fossils from various European localities to show that the dicotyledons were not exterminated in the Cenomanian ; and there are traces of plants in the Upper Cretaceous of the mid-Tethyan region which support the view that tropical angiosperms were established there before the Eocene : leaf impressions from Senonian to Maestrichtian beds of Syria, Persia and Iraq (at present being studied by Dr. E. Boureau and myself) are strongly reminiscent of the mangrove *Aegiceras* (Myrsinaceae). There are, therefore, some grounds for believing that the forerunners of the Eocene Poltavian flora had a circum-Tethyan distribution in the later Cretaceous, so that some elements at least of the British London Clay flora did not necessarily have to make the six thousand miles journey from the Far East in the middle Eocene. Indeed, some of the few early Tertiary (pre-Ypresian) plants from southern England are essentially tropical : the fern *Lygodium*, for example, recently

reinvestigated by Chandler, and some silicified palm stems. On the other hand, western Europe has undergone so many local geological changes that one cannot expect the same vegetational stability that would be found, say, in parts of the great Asian land mass, and the early Eocene may quite possibly have witnessed that accentuated floral immigration which Chandler postulates.

Further light might be thrown on plant distribution in the Tethyan region if the intertrappean plants of the Deccan were more fully investigated. The existence of this flora, which includes fruits, seeds and even flowers beautifully preserved in chert, has been known for more than a century, but it has never been fully described, partly, no doubt, because of the great difficulty of interpreting the material. Before he can identify such fossils even approximately, the palaeobotanist is compelled to study a wide range of the comparable parts of recent plants. The age of the Deccan intertrappean flora is still uncertain, whether Upper Cretaceous or Eocene. It is said to include *Nipa*, and this has been claimed as supporting an Eocene age, but homotaxial floras are not necessarily synchronous. Some striking members of this assemblage have been anatomically investigated in recent years, but few of them have counterparts in other fossil floras and few of the angiosperms (apart from some palms) have so far been closely linked with any living plants. Admittedly the plant evidence so far as it goes rather favours an Eocene age, but this is just one of those cases in which the phytogeographer would like to be absolutely certain of the age. In the lower Deccan traps there are marine intercalations—the Rajahmundry limestones—and interestingly enough these contain a considerable variety of calcareous algae, unfortunately not very well preserved, which Pia thought added to the probability that the traps in this area were erupted in Tertiary and not Cretaceous times. Although in this address land floras have been under consideration, it is worth drawing attention to the assistance in dating Tethyan deposits which may one day be available when the Mesozoic and Tertiary calcareous algae of the Tethys are more fully worked out. Many of these algae are from rocks of which the age is known, as in the Paris basin and in the Danian of southern India, and a recent important addition to our knowledge is due to G. F. Elliott, who has described a considerable range of algae from Mesozoic and Eocene beds of Iraq.

Another region from which further fossil plant evidence is urgently required is Africa. Here is a land area in which successive tropical floras might have flourished, traversed in its northern part by a belt which has been periodically, but not permanently, either dry or actually desert. At Air in the central Sahara grew the Upper Devonian or Carboniferous *Lepidodendropsis* flora, and Jongmans thought that somewhere in Africa the missing Upper Carboniferous tropical flora might be found. (If it is, it will most probably resemble the Euramerian Westphalian.) From the Permian onwards through the Mesozoic there are isolated fossil plant occurrences which cannot yet be combined to give a picture of the floristic succession, and, above all, the phytogeographer would like to know more of African angiosperms of the past. Fortunately, a most important and extensive flora has been found during the last few years on Rusinga Island in Lake Victoria by Dr. L. S. B. Leakey. Probably of Lower Miocene age (a little later, perhaps, than the palaeobotanist might have desired) it should when fully worked out greatly increase our knowledge of Tertiary tropical floras and throw light on many phytogeographical problems.

The final phase of tropical Poltavian expansion came in the Oligocene. A tremendous cycle of earth movements and mountain building then again ushered in a glacial period. The Tethys sea was reduced to the present Mediterranean and the Poltavian flora was exterminated in Europe, surviving only in that south-east Asian region where it probably originated. The early Tertiary Arctic floras extended southwards in western Europe, and the Asiatic Turgaian flora of which they were an extension colonised eastern Europe. A colonising flora from the north and north-east in a cooling climate would not be rich in species, and the European flora was further impoverished as the glacial period developed. The present temperate

flora of the Chinese region is vastly richer because it is an original Tertiary flora with many late Cretaceous survivors, and perhaps with some later immigrants from the holarctic region as well as admixtures from the south.

Throughout the Tertiary the climatic latitudinal zones in the northern hemisphere are again most apparent. I have been unable to include any account of the North American angiosperm floras, the general floristic character of which is well known, and follows a very similar distributional pattern to those of the Old World. I would draw attention to Chaney's *isoflors*, as he calls the lines or belts of vegetation of similar type which lived in similar climates. He connects those of North America with those of Europe and Asia, and thus again demonstrates the vegetational and climatic zonation from the tropics to the north pole. To explain the distribution of the flowering plants from the mid-Cretaceous to the present day, Chaney sees no reason to invoke the aid of continental drift. As he says, 'forests under compulsion of climatic change, rather than the continents on which they live, appear to have been the wanderers during the history of life upon the earth.' Or, to put it another way, when Birnam Wood came to Dunsinane it moved by organic agency and not by crustal drift.

Quite recently a new theory of polar wandering has been propounded, based partly on the instability of the earth's axis of rotation and partly on what has been called the 'fossil magnetism' of the rocks. The earth's rotational axis is not fixed, but exhibits various changes of direction, the major movement of the precession, for example, and the minor oscillation known as nutation—the dance, one might say, 'at the still point of the turning world.' There are, however, in Dr. T. Gold's words 'other effects . . . produced . . . by a redistribution of matter or angular momentum on or within the earth. They are movements whereby the direction of the axis of rotation remains nearly fixed in space, while the earth moves relative to it.' Dr. Gold discusses the long-term stability of the axis, and considers those tectonic movements which might in certain circumstances cause the earth to topple over. He then links that possibility with occasions in past geological time when there have been drastic changes of climate. Here geophysicists who have been measuring 'fossil' magnetic direction come in, and report that at such and such periods in the past the direction of the earth's magnetic axis has been greatly different from that of to-day. Assuming then that the magnetic axis coincides with the rotational axis, they proceed to plot the presumed positions of the poles at various periods. (In a sense, of course, it is not the pole which moves, but the earth itself in relation to the more or less static pole.) It is too early yet, since little has been published on the magnetic data, to say how the theory will fit the geological and palaeontological record. Dr. Gold claims that 'the wandering of poles over great distances would fit in well with all that is known about the earth, and would re-affirm what can already be inferred from other data'. If this should prove to be true, then all I have said here is no more than a tale told by a palaeobotanist, signifying nothing. The conclusion, however, would rather seem to be that the past history of plant communities on land does indicate a continuous arrangement of the land masses, both in relation to each other and to the equator and poles, roughly corresponding with that of the present day. Any attempt to sketch this history in short compass must involve too many unqualified generalisations, too few detailed analyses, and a too superficial and impressionistic treatment of the changing picture. In brief, the essay suffers from the same defects as the palaeogeographic map. To fill in essential details there is plenty of material for students of palaeobotany who will devote the time and patience to studying it, to say nothing of floras still to be discovered in many parts of the world. Even now a more balanced and coherent account might be drawn up by anyone who would read and sift critically the data already published.

Darwin, in a letter to J. D. Hooker, once wrote, 'I have often thought that science would progress more if there were more reading'. It was an old subject of grief to him, he said, 'that the very best men read so little . . . and give up nearly their

whole time to original work'. For those of us who do not have to bear the burden and the responsibilities of profound originality, it is equally important that we should read more, and it might be as well if we wrote less. And if we must write, let us at least endeavour to be brief and clear ; otherwise Darwin's injunction will before long become an impossible task, and future generations will search well-nigh in vain for the few distorted fossils of fact or fancy in a vast sediment of words.

IMMUNITY AND DEVELOPMENT

Address by

PROF. F. W. R. BRAMBELL, F.R.S.

PRESIDENT OF SECTION D

THE science of zoology occupies a central position in the group of subjects which constitute animal biology. It is the parent subject from which spring logically all the others, including those such as human anatomy, human physiology, etc., which deal primarily with man. Many of the branches of human biology, because of their medical importance, have developed in advance of the corresponding comparative subjects of which they are parts. Sometimes the medical aspect has progressed so far in advance of the comparative subject as to lead to the neglect of the latter by zoologists unwilling to enter a field already so specialised. Whenever the human and comparative aspects of a subject have failed to develop together, for this or other reasons, it has been to the disadvantage of both. It is the proper and essential function of zoology to develop the comparative studies on which such specialisms should be based; indeed, it should provide the central co-ordination of the whole group of subjects comprising animal biology. This is a function of immense importance. The specialism can derive stimulus and inspiration from the comparative studies. The zoologist can fill the gap between two specialised branches and show how they are interrelated. The borderland often proves to be a field of research in which rapid advance is possible by building on the knowledge and using the techniques of the adjoining subjects. One such field that has proved fruitful in recent years is that between immunology and embryology. It is to recent advances in this field, particularly in relation to mammals, that I propose to devote my address.

The relationship of mother and young in mammals is one which involves special immunological problems. The retention of the developing young in the body of the mother and in organic continuity with her tissues during pregnancy must be effected without the development of those immunological reactions which result normally from the introduction of tissues, cells or proteins of one individual into the system of another. Reactions resulting from immunological incompatibility could cause severe damage or death of the foetuses, perhaps of the mother also. It is only recently that we have begun to understand the ways in which such reactions are normally avoided and the abnormal circumstances in which their occurrence causes disease of the newborn or death of the foetus.

Consider some of the ways in which an adult mammal responds to the introduction into its system of proteins, cells or tissues derived from another animal.

Serum protein, when injected into another species of mammal, such as a rabbit, induces an immune response which results in the formation of circulating antibodies capable of reacting with the foreign protein. Immune serum containing these antibodies, when mixed with the foreign serum protein solution employed as antigen, will react and produce a precipitate. Antibodies which precipitate the soluble antigen in this way are called precipitins. Although a mammal will not react immunologically to the injection of serum proteins derived from another member of its own species, it will react to those of another species, even of a nearly related one. Generally, however, more potent antisera are produced to antigens derived from distantly related species. The immune sera resulting react more intensely with serum protein of the same species as that from which the antigen was derived, though they will react also with

serum proteins of other species related to that from which the antigen was derived. Nuttall (21) was a pioneer in this field just over half a century ago. He was able to show that the intensity of such cross-reactions, as they are called, can be used as an approximate measure of phylogenetic relationship between the species.

It was discovered by Landsteiner (24) that the sera of some human beings, when mixed with the red blood corpuscles of others, caused the corpuscles to become clumped or agglutinated. This was shown to be due to the presence of antigens on the surfaces of the corpuscles and of the corresponding antibodies or isoagglutinins in the serum. So the ABO blood groups, the first of the blood groups to be recognised, were discovered. Subsequently other human blood-group systems were found and today we know of nine (24). Each group is characterised by the presence of specific antigens on the surfaces of the red blood corpuscles. These antigens are inherited in a simple Mendelian manner as allelomorphs at a single locus or at closely linked loci (Rh-groups). Of the nine blood groups known, seven appear to be inherited independently of the other two, between which linkage has been detected. The ABO blood groups are unique in that those individuals which have not got the antigen (A or B) on their red blood corpuscles almost invariably have the corresponding isoantibody present in their sera. Obviously, the antigen and corresponding isoantibody are never present together in the blood of any normal person, for such an individual would not survive. All the other blood groups are characterised by the antigens borne on the red blood corpuscles; the corresponding isoantibodies normally being absent in individuals lacking the antigens, although they can be developed in response to active immunisation. Such active immunisation may result from blood transfusion or, in the case of certain blood groups, naturally during pregnancy by transference from the foetus to the mother. The isoantibodies are not normally developed without such immunisation, even when the corpuscles lack the corresponding antigen. Some of the antigens are so weakly antigenic that isoantibodies are developed rarely or not at all even under favourable conditions of immunisation, and antisera to these antigens produced in other species of animals must be employed for their recognition. The number of possible combinations of antigens belonging to these nine blood groups is so large that the chances of any two individuals, selected at random from a population, having similar blood-group antigens is small. The blood groups can serve to distinguish an individual from most other members of the human species.

Blood groups occur also in many other animals (13). Amongst the Primates, groups resembling the ABO and MN groups of man are found and the human Rh-group derives its name from an antigen present on the red blood corpuscles of the rhesus monkey. In the horse, ass, cow, sheep, goat and pig, antigens are present on the red blood corpuscles and appear to be inherited in a simple Mendelian manner. Isoagglutinins are present sometimes, though not invariably, in the serum when the corresponding antigen is absent. At least six antigens are known in the horse and as many corresponding isoagglutinins. The isoagglutinins, when present naturally, tend to be at very low titres, but these may be increased by immunisation with red blood corpuscles carrying the appropriate antigens. The variety of combinations of antigens and of isoagglutinins encountered is so large that it is exceptional to come across two horses with identical blood groups. Donkeys have some antigens which are not shared with horses, as well as those which are common to both. Mules have some which are shared with the horse but not with the donkey, and at least one shared with the donkey but not with the horse. In cows the position is complicated, for there are upwards of one hundred antigens known in two principal blood-group systems. It is probable that not all of these are inherited independently. Isoagglutinins also occur in bovine serum. In the sheep and in the pig there are at least three groups determined by the presence of an antigen on the red blood corpuscles or by its absence with or, alternatively, without the corresponding isoagglutinin in the serum. The antigen, in both sheep and pigs, has certain similarities to human A substance; for example, human corpuscles of group A

will absorb the isoagglutinin from pig serum, and pig corpuscles carrying the antigen will absorb the isoagglutinin from human serum. There is evidence of the occurrence of several other antigens, in both pigs and sheep, demonstrable with suitable immune sera. The dog has five antigens, of which four appear to be inherited independently of each other. Isoagglutinins for each of these antigens are known and may or may not occur in the serum of those individuals lacking the corresponding antigen. The cat, also, has isoagglutinins which distinguish at least two antigens. Two antigens have been recognised on the red blood corpuscles in rats. These are inherited in a simple Mendelian manner independently of each other ; the presence of the antigen being dominant to its absence in each case. Consequently a rat may have both, either or neither of these antigens present on the corpuscles. The isoagglutinin to one of these antigens is present normally in the serum of animals lacking the antigen, but the isoagglutinin to the other antigen is developed only after immunisation of animals lacking the antigen. Two antigens have been recognised in rabbits also. These are inherited as allelomorphs dominant to a third allelomorph for which no antigen has been recognised. Consequently rabbits may have both antigens present together, either alone, or neither on their red blood corpuscles. Isoagglutinins occur in nature but are rare, though they can be produced readily by isoimmunisation. Undoubtedly other antigens also occur on the red blood corpuscles. Blood groups occur also in birds and have been studied in fowl, ducks and doves. Hence, it appears probable that blood groups may be found to be of general occurrence in mammals and birds.

A tissue from one individual mammal can be grafted on to another of the same species, where it will grow and survive, for a time at least (4). Skin provides a particularly favourable tissue for grafting, because it can be transferred to a precisely similar position on the body to that from which it has been taken, without serious damage to the vascular supply, and because, on account of its superficial position, its survival after grafting can be observed so readily. It is also more sensitive to immunological differences between recipient and donor than are other tissues which can be grafted. Skin derived from another member of the same species will not normally survive permanently unless (*a*) both recipient and donor are identical twins, (*b*) the donor is a member of a highly inbred line to which the recipient belongs or of which it is the F_1 offspring, or (*c*) the recipient has been rendered incapable of so reacting, which can occur in special circumstances to be considered presently. Although, when these conditions are not met, the skin graft may take and survive for a time, it will ultimately be destroyed by the recipient as a result of an immune reaction. The severity and time of onset of this immune reaction will depend on the degree of antigenic diversity between recipient and donor and on whether the recipient has been immunised previously to the antigens of the donor. Thus, skin grafting provides a very sensitive test of antigenic diversity, the grafted skin being capable of overriding only very minor antigenic differences (4).

These three examples show how an adult responds to the introduction into its system of tissues, cells or proteins from another species, or even from another individual of the same species. It will react to the introduction of serum proteins from another species, but not to those of its own. It will react to the introduction of red blood corpuscles from another member of the same species but of different blood group; that is to the red blood corpuscles of most other members of the species. It will react to a skin graft from any other member of the species, except in very special circumstances. These reactions are an expression of the individuality of the adult organism.

The capacity to respond immunologically to the introduction of antigens into the system by the production of circulating antibodies appears relatively late in development and is achieved gradually. It is well known that mature mammals produce more potent antisera than young ones. Circulating antibodies cannot be produced in detectable amounts by active immunisation of new-born animals. Before birth the mammalian foetus appears to be tolerant of red blood corpuscles of

different blood groups, but the capacity to react to skin grafts from another individual is developed even in the foetus in sheep, cattle and guinea-pigs (26). Foetal lambs of 80 days' gestation display a strong reaction to such grafts. Anastomoses often occur between the blood vessels of the foetal membranes of twin calves, thus joining the circulations of the twins and permitting interchange of blood cells between them (22). Provided this occurs sufficiently early in development, blood-forming cells, as well as red blood corpuscles, may be interchanged. When such twins are of different blood groups, each has, in addition to its own red blood corpuscles, a definite proportion of those of the twin. Not only are such twins viable but the condition is permanent, the proportions of the two types of red blood corpuscles remaining constant throughout life owing to the continued production of both types by the blood-forming tissues. This phenomenon occurs also in sheep (28). Vascular anastomoses are known to occur sometimes between human twins produced from two eggs, as well as between identical twins. Thus, the possibility of blood-group chimaeras, like those in cattle, exists. Recently (14), a woman was discovered who had 60 per cent. of her red blood corpuscles belonging to group O and 40 per cent. to group A. On inquiry it was found that she was one of a pair of twins which were not identical, the other having been a boy who had died in infancy. She was a secretor, producing O(H) substance, but not A substance, in her saliva, thus indicating that her genotype was O. The probability that the A corpuscles were derived from the twin during foetal life is evident. Occasionally double-yolked eggs occur from which twin chicks can be hatched. Such twin chicks are joined by vascular anastomoses between their allanto-chorionic membranes within the egg. When hatched they prove to be red blood cell chimaeras and fully tolerant of reciprocal skin grafts (Brent, personal communication).

Many examples of the tolerance of embryos to grafts of tissues can be found in the literature of experimental embryology. The chick in particular will tolerate grafts not only from other individuals of the same species but also from other species of even distantly related groups. Mammalian, as well as avian, embryonic tissues (25) and tumours (19) from a variety of sources can be grown in this way. The chorio-allantoic membrane on the eighth day of incubation provides a particularly favourable site for such grafts. The grafts are vascularised by the allantoic vessels of the host and continue to grow until hatching, without apparent reaction on the part of the host.

It may be relevant in this context to remember that at a still earlier stage of development the zygote nucleus comes into being in an environment of maternal cytoplasm. The cytoplasm of the egg must contain maternal proteins that would be antigenic in the adult offspring, yet it is in this medium that the zygote nucleus must function and initiate the synthesis of all those proteins which will characterise the new organism. These new proteins will, during development, dilute and gradually replace those of maternal origin. The maternal, as well as the embryonic, proteins will be metabolised and, not being replaced, will tend to disappear. How long they will persist is uncertain but it may be assumed that they will have gone at the time when the young animal is capable of reacting immunologically to them.

These examples show that the foetus and the young animal for some time after birth are tolerant of foreign proteins, cells and tissues to which they would react immunologically when adult. This immunological inactivity obviates the risk of the foetus reacting to incompatible maternal antigens during pregnancy but leaves it unable to protect itself against infection before, and immediately after, birth. Consequently temporary protection is afforded to the young by the passive transference of antibodies from the mother, as was shown first by Ehrlich (7). In birds transference takes place by way of the yolk of the egg, maternal circulating antibodies being secreted by the ovary into the egg-cell as livetin. The antibodies persist in the yolk throughout incubation and are absorbed by the yolk-sac wall, appearing in the circulation of the chick. Absorption takes place late in incubation and during the first two or three days after hatching (8). In mammals transference may occur before birth, by way of the uterus

and the foetal membranes, and, after birth, by way of the mammary secretions and the gut of the young (7). In the cow, sheep, goat, horse and pig the young are born without any antibodies in their blood and transference occurs entirely by way of the colostrum. Antibodies are present in the colostrum at concentrations equal to or greater than those in the mother's blood and these are rapidly absorbed by the gut of the new-born young and appear in the circulation, at concentrations equalling those in the colostrum, during the first few hours of life. The capacity of the gut to absorb the antibodies is transitory and is lost after about a day. In dogs a small amount of transference appears to occur before birth but the greater part occurs after birth by way of the colostrum and the milk. The capacity to absorb antibodies from the milk persists in the puppy for 10–12 days. In rats and mice antibodies are transferred in significant amounts during pregnancy but the greater part of the transference occurs by way of the colostrum and the milk. Transference by this route continues actively throughout lactation and then ceases comparatively suddenly at 21 days in the rat (15). In the guinea-pig, rabbit and man transference takes place before birth and there does not appear to be any significant transference after birth.

The capacity of the mother to develop active immunity and to transfer her circulating antibodies to the young involves a risk should she produce antibodies to antigens possessed by the young. In these circumstances the maternal antibodies would react with the cells of the young and disease might result from the immunological incompatibility. This has been found to occur, both in man and in some domestic animals, and to result in haemolytic disease. It is due to the mother becoming immune to certain blood-group antigens borne on the red blood corpuscles of the offspring and inherited from the father. The antibodies produced by the mother, when transferred to the offspring, react with the red blood corpuscles, which are destroyed.

Haemolytic disease was recognised first in man (20) and was found to be due to the mother being immunised to an antigen of the Rhesus blood groups which she did not possess, but which the infant had inherited from the father. The blood-group antigens vary greatly in their capacity to stimulate the production of antibodies in individuals lacking them (24). The Rhesus antigen D is by far the most potent in this respect, although antigens of other human blood groups are known to be responsible for haemolytic disease. Persons possessing antigen D are said to be Rh-positive and those lacking it Rh-negative. Immunisation of Rh-negative persons occurs most readily by blood transfusion of Rh-positive blood, although some people react more than others. Immunisation of a Rh-negative woman may result also from bearing one or more Rh-positive children, but this occurs by no means regularly. Once the mother has been immunised, either by transfusion or child-bearing, she will transmit her antibodies to all subsequent children while they are still in the uterus. Should these be Rh-positive, damage to the red blood corpuscles will result almost invariably, producing haemolytic disease. This may cause death of the infant before or soon after birth, permanent damage, or transitory symptoms which may be so slight as to be scarcely recognisable. Haemolytic disease rarely affects a first-born child, unless the mother has been immunised by previous transfusion. It appears that the mothers vary widely in the strength of their immune reaction to the antigen, that pregnancy with an incompatible foetus is an erratic means of effecting immunisation, and that the severity of the disease by no means depends simply on the concentration of antibodies in the maternal blood. The antibodies produced are of more than one type and differ in the ease with which they are transferred to the foetus and in their capacity to destroy foetal red blood corpuscles.

Haemolytic disease was recognised next in horses. It was well known to breeders that new-born mules were liable to suffer from fatal haemolytic jaundice. This was shown (12) to be due to immunisation of the mare by antigens of the donkey and to the transmission of the maternal antibodies to the new-born mule in the colostrum. The mule is born unaffected, even when the mother is immune, but gets the antibodies with the first feed and rapidly

develops the disease. Thus, if the new-born mule is removed from the immune dam before it has fed and if it is fostered on a non-immune mare, it remains unaffected. A precisely similar disease has been found to occur in thoroughbred foals (9), due to the mare having become immune to antigens shared by the sire and the foal, but which she lacks. Again, the foal is unaffected at birth and develops the disease after the first feed. The disease can be avoided by fostering the foals inheriting the antigen to which the mares are immune or by serving these mares subsequently only with stallions which do not possess the antigen.

Recently a similar condition has been identified in pigs (11) in which the piglets are born healthy, develop the disease after 48 hours and usually die by the fifth day. Their blood corpuscles were coated with maternal antibody, and the mother's serum reacted with the red blood corpuscles of the boar, thus establishing the nature of the disease beyond doubt.

These, so far as I am aware, are the only animals in which disease due to maternal/filial immunological incompatibility naturally occurring has been established beyond reasonable doubt up to the present. It is to be expected that haemolytic disease will be found to occur in other animals. It is quite possible that the mother could become immunised to foetal antigens other than those borne by the red blood corpuscles, resulting in incompatibility diseases of kinds not yet recognised as such.

Antibodies are formed only in animals which have been immunised and they are highly specific, reacting only with the particular antigen which provoked their formation. Apart from this property of reacting with the specific antigen, the antibody molecules behave as part of the globulin fraction of the serum proteins. Consequently, antibodies formed in one individual can be injected into another, non-immune, individual and can be identified subsequently by their reactivity with the antigen. Hence their movements in the non-immune animal can be followed as though they were marked molecules. It has been possible in this way to trace the route by which antibodies normally pass from the maternal to the foetal circulation in rabbits and to test the permeability of the various foetal membranes to them (7). It has been shown by this means that in rabbits at any rate antibodies do not pass from the maternal to the foetal circulation by way of the placenta, as was believed previously. Instead, they pass from the maternal circulation into the uterine cavity, from where they are absorbed by the entoderm of the inverted splanchnic wall of the yolk-sac and are transported by the vitelline circulation. Antibodies also pass from the uterine lumen through the membranes into the exocoel and from there through the amnion into the amniotic fluid. They are swallowed with the amniotic fluid and become concentrated in the gut, but they do not pass in significant amounts from the gut into the circulation of the foetal rabbit.

These results raise again the problem of the route by which maternal antibodies reach the human foetus. Since the placenta is impermeable to them in the rabbit, it may be so also in the human (6). The late human conceptus is bounded by the placenta (*chorion frondosa*) and by the smooth membrane of fused chorion and amnion (*chorion laeve*). Hence, if the antibodies do not enter by way of the placenta it would seem that they must do so through the smooth membrane, thus reaching the amniotic fluid and presumably the foetal gut. Whether they would be absorbed in the gut and enter the circulation, as occurs in the new-born young of those species in which transference is by way of the colostrum, or whether the gut is impermeable, like that of the foetal rabbit, is problematic.

The passage of antibodies through the foetal membranes can be studied conveniently in rabbits by introducing the immune serum into the uterine cavity, thus directly exposing the foetal membranes to it. This by-passes the tissues through which antibodies from the maternal circulation would have to pass to reach the uterine lumen. It is possible to use immune sera prepared in other species as well as those prepared in the rabbit. Provided these are specific for different antigens and so can be distinguished, the sera can be mixed and the foetus exposed to the mixture. Thus the entry of antibodies produced by different species

can be compared directly. It has been found (2) by this means that antibodies produced in rabbits enter the circulation of foetal rabbits more readily than those produced in other species. Measuring the entry by the concentration attained after a standard time of exposure, it was found that the species that had been used to produce the immune sera formed a series—rabbit, man, guinea-pig, dog, horse, cow, in descending order. This selective passage of antibodies from the uterine lumen into the foetal circulation is a very interesting property of the yolk-sac wall of the foetal rabbit, perhaps the more so because the passage into the amniotic fluid is not selective, nor is that from the uterine lumen into the maternal blood, antibodies attaining equivalent concentrations in these fluids irrespective of their species of origin. This selectivity of immune globulin by the yolk-sac of the foetal rabbit recalls that displayed by guinea-pig smooth muscle in passive anaphylactic sensitisation. Smooth muscle exposed to immune serum either *in vivo* or *in vitro* becomes sensitised and then will react immediately to the presence of the specific antigen by maximal contraction. Hartley (17) has shown that diphtheria antitoxin prepared in guinea-pigs, rabbits or man will sensitise guinea-pig smooth muscle in this way, whereas diphtheria antitoxin prepared in horses, pigs, cattle, goats or sheep will not sensitise.

The passage of antibodies from the yolk to the blood of the newly hatched chick takes place through the wall of the yolk-sac (8). This membrane appears to be selective also, because antibodies prepared in pigeons enter the circulation less readily than those prepared in fowl, and antibodies prepared in rabbits or cows appear not to enter. Again, the passage of antibodies from the gut to the circulation of young rats is selective (15), for antibodies prepared in mice enter nearly as readily as those prepared in rats, but antibodies prepared in rabbits enter significantly more slowly than either, while those prepared in cows or in fowl do not enter in significant amounts. It will be noted that in all these three cases the selective passage is through membranes which are morphologically comparable, being entodermal surfaces of gut wall, embryonic or extra-embryonic. There is some evidence that the passage of antibodies to the human foetus may be selective also, for although maternal diphtheria antitoxin reaches it readily, purified diphtheria antitoxin, prepared in horses and administered to the mother, does not (18). Moreover, some kinds of maternal Rh-antibodies appear to reach the foetus more readily than others. This suggests that an entodermal membrane, which could only be the gut wall, is involved in the passage to the human foetal circulation also. It lends support to the suggestion that the smooth membrane (*chorion laeve*), amniotic fluid and gut may be the route, rather than the placenta (6).

Antibody production takes place normally only in response to the introduction into its system of antigens which the animal itself lacks. For example, an animal's own red blood corpuscles do not induce the formation of antibodies in its own blood, though they would do so in other individuals not of identical blood groups, nor does the grafting of an individual's own skin induce an immune reaction. With the human ABO blood groups, individuals lacking one or both of the antigens normally produce the corresponding antibody (isoantibody) without being immunised, though individuals possessing the antigens never produce the corresponding antibodies. Much impressed by this anomaly, Burnet and Fenner (10) suggested that the presence of the antigen at early stages of development permanently modifies the antibody-producing mechanism in such a way as to suppress the production of that particular antibody. This idea is proving productive. At the time, the existence of permanent blood-group chimaeras in two-egg twin calves had been demonstrated (22). If exchange transfusion, of the kind that had occurred in these during foetal life, occurred in adult life between animals of different blood groups, each would have been immunised to the blood of the other, yet in these chimaeras no antibody to the red blood corpuscles of the twin was produced. Still more remarkably, the human blood group chimaera referred to above (14), although genotypically group O and, thus, to be expected to have antibodies both to the A and B antigens

(isoantibodies alpha and beta), did not produce the antibody to A, the antigen possessed by the corpuscles derived from the twin.

Skin-grafting experiments have thrown further light upon the nature of two-egg twin calves (1). Reciprocal skin grafts between twins were performed in the expectation of being able to distinguish by this means between one-egg and two-egg twins. To be able to do so with certainty would be of value because one-egg (identical) twins are required for agricultural research. It was expected that although grafts between identical twins would survive indefinitely, those between the two-egg twins would break down. It was found, however, that grafts exchanged between pairs of twins survived indefinitely, irrespective of whether these were one-egg or two-egg twins. Even after repeated reciprocal grafts between two-egg twins, the grafts survived. Reciprocal grafts between full siblings of different births, however, broke down rapidly, although genetically such siblings do not differ more than two-egg twins. These results strongly suggest that exchange of antigens in foetal life has resulted in permanent tolerance to reciprocal skin grafts as well as to the red blood corpuscles of the twin.

This led naturally to attempts to produce tolerance experimentally. It was found (3) that by injecting a small amount of a suspension of cells prepared from an adult mouse of one inbred strain into the 15–16-day embryos of another inbred strain the latter acquired permanent tolerance to skin grafts from the former. Similarly, with fowl, transfusion of whole blood from one embryo to another at 11–12 days of incubation resulted in the recipient being tolerant of skin grafts from the donor. This acquired tolerance, whether in mice or fowl, is highly specific. For example, mice rendered tolerant to one highly inbred strain will react normally to skin grafts from any other strain. With fowl, in which highly inbred strains are not available, it was necessary to use skin grafts from the same individual donor as had provided the blood transfusion, if tolerance was to be demonstrated. That this tolerance is not due to alteration in the antigenic capacity of the graft to provoke a response but to inability of the host to react immunologically to its presence can be shown by passive transfer of immunity in mice. Established skin grafts on tolerant hosts can be destroyed by intraperitoneal inoculation of chopped lymph nodes from immune mice of the same strain as the host, which had been immunised to skin of the donor strain. It appears that although foetal mammals and embryo chicks cannot produce antibodies, they can respond to the presence of the antigens of foreign cells in a way that permanently prevents the formation of antibodies to these antigens. It is important to note, however, that in these experiments living cells were injected and probably survived in the host, as is the case in natural blood group chimaeras in twins. Tentatively, it appears that when living cells are transplanted to another individual before it has developed the capacity to react immunologically to a graft (homograft), they may survive and render the host permanently incapable of reacting to their antigens. It is evident that the antigens must be introduced into the system before the development of the capacity to react to them. In chimaeras, natural or artificial, the graft cells persist until the animal is immunologically adult and capable of reacting normally to other antigens.

It is well known that Rh-negative people react erratically to Rh-antigen. It was suggested that this might depend on the blood group of their mothers, the children of Rh positive mothers being rendered tolerant to the Rh-antigen in foetal life. Hence a Rh-negative woman who was the daughter of a Rh-positive mother might be less readily immunised by her own Rh-positive infant than one who was the daughter of a Rh-negative mother. Consequently, the Rh-groups of the maternal grandmothers of children suffering from haemolytic disease were investigated (5) and it was found that the ratio of Rh-positive to Rh-negative grandmothers did not vary significantly from that of the population as a whole and hence did not support the suggestion. Similar results were obtained in America (23), but it was found that when the occurrence of Rh-antibodies in the maternal circulation, rather than haemolytic disease of the

infants, was investigated a marked correlation with the blood groups of the maternal grandmothers was revealed. A total of 51 Rh-negative mothers, none of whom had ever had a blood transfusion, were divided into two groups according to whether they had, or they had not, developed Rh-antibodies before or during their third pregnancy with a Rh-positive child. It was found that of the 34 who did not develop antibodies, 25 were the daughters of Rh-positive women, whereas of the 17 who did develop antibodies only 4 were the daughters of Rh-positive women, a distribution that could happen by chance in less than one in a hundred times. Thus, it appears that the Rh-negative daughters of Rh-positive women have acquired a partial tolerance to Rh-antigen. It is difficult to believe that in this case the acquired tolerance, if it should prove to be such, is due to the entry and survival of Rh-positive maternal cells in the Rh-negative daughters. It is much more probable that the partial tolerance is due to a transitory exposure of the daughters to maternal antigens while they were still in the uterus and that they are not permanent cell chimaeras.

Hanan and Oyama (16) treated pregnant rabbits during the second half of gestation with bovine serum albumin injected intravenously. Some of the young from each litter were given three injections per week of bovine serum albumin for a period of 16 weeks. The doses were of a magnitude suitable for inducing immunity. These animals did not develop immunity to bovine serum albumin, judged by precipitin tests *in vitro*, by passive anaphylactic tests in guinea-pigs, or by skin tests in the rabbits themselves, either during the course of the treatment or after its cessation. Thereafter, immunity could not be induced by further injections of the antigen. Moreover, the capacity of these rabbits to develop immunity to crystalline egg albumin was reduced significantly. It appears that they had developed an acquired tolerance to bovine serum albumin and a partial tolerance to crystalline egg albumin. Other young from the same litters were retained as controls until they were 16 weeks of age and were then given a series of three intravenous injections per week for 12 weeks. These young all developed active immunity, giving strongly positive reactions to precipitin, anaphylactic and skin tests. It would appear that administration of the antigen to the mother during pregnancy was not sufficient alone to establish tolerance in the young, nor could it be established by a long series of injections after 16 weeks of age. Continued administration of the antigen from birth to maturity appeared to be essential to produce tolerance, which then persisted when the treatment was discontinued. It will be noted that the tolerance in this instance is to a foreign antigen, not to an isoantigen, and that it is a soluble antigen.

It is found that when tumours are transplanted from one inbred strain of mice to another, some strains are susceptible and others resistant. The tumours grow in susceptible mice and kill them, whereas resistant mice survive. Snell (27) has shown that mice of resistant strains could be rendered susceptible by treatment with a course of injections of a saline suspension of freeze-dried and powdered tissue from a member of the donor strain prior to grafting the tumour. Normal tissue was as effective as tumour tissue when prepared in this way, and kidney tissue was more effective than liver or spleen. The effect was highly specific in relation to the donor strain. The freeze-dried tissue suspension must be administered at least one day before grafting for the effect to be fully displayed, but, once established, it persists without diminution for at least 19 weeks. Freeze-dried tissue derived from other species (rats, hamsters, and guinea-pigs) has no effect in mice. The effect can be passively transferred from a treated to an untreated mouse by transfusion of serum. Serum from rabbits that were treated with freeze-dried mouse tissue, when transfused to the host mice, was effective also. It had been shown that in mice two of a series of seven allelomorphic genes produce factors which are particularly effective both as blood-group isoantigens and in relation to tissue graft resistance. The various inbred strains of mice employed by Snell and his colleagues could be divided into four groups according to whether they possessed one or other, both or neither of these factors,

named D and K. Host strains possessing D, though they were normally almost completely resistant, could be rendered susceptible of tumour grafts from DK donors by treatment with freeze-dried tissue from any K strain, so that 55–62 per cent. were killed by the growth of the tumours. Treatment with freeze-dried tissue from any strain lacking the K factor left them as resistant as before, only 1 per cent. or fewer dying of the grafted tumour. Similarly, host strains possessing the K factor, though normally completely resistant (3 per cent. dying), could be rendered susceptible of DK tumour grafts by treatment with freeze-dried tissue from D strains; of such animals 60–62 per cent. died from the growth of the implant. In hosts possessing K, however, treatment with freeze-dried tissue from strains lacking D appeared to confer a significant, though lesser, degree of susceptibility, for 21–22 per cent. died of the tumour. The factor in the tissues capable of inducing susceptibility was found to be present in washed mitochondria and microsomes.

This phenomenon, at least superficially, resembles in some respects acquired tolerance as defined heretofore. It is produced by treatment with an isoantigen and it appears to result in a reduction of the immune reaction to grafts. It differs profoundly, however, in that the treatment is effective in the adult animal and does not require to be administered to the foetus. Moreover, it is important to note that the effect is measured by the establishment and growth of malignant tumours which are rapidly fatal, a test which is much less exacting than that of the permanent survival of grafts of normal tissues. Why injection of freeze-dried tissue preparations into the adult animal should result in susceptibility, rather than increased resistance, is difficult to understand, but it would be premature to theorise further as to the nature of the mechanism involved.

This address must now be brought to an end. It has ranged widely over a somewhat heterogeneous collection of researches: blood groups in man and animals, skin grafting, the tolerance of the embryo to grafts, blood-cell chimaeras amongst two-egg twins, the endowment of the young animal with its mother's immunity, haemolytic disease of the new-born, the route of transfer of proteins from mother to foetus, the selective passage of proteins through the entoderm, and experimentally induced tolerance to antigens. Selected topics were confined to comparatively recent work which appeared to advance both mammalian embryology and immunity, except in so far as it was necessary by way of introduction to include older work. These studies have thrown new light on the functions of the foetal membranes in mammals and on the causes of much prenatal and neonatal mortality. They have shown that the cells of the foetal membranes can selectively transport globulin according to the species in which it was synthesised. This selectivity has some resemblance to other known immune responses, such as passive anaphylactic sensitisation of smooth muscle, but the mechanism by which it is effected remains to be elucidated. It is exercised at a much earlier stage of development than that at which the organism can produce circulating antibodies. Likewise, in actively acquired tolerance, we have evidence of the embryonic cells being permanently modified by exposure to specific antigens at a stage long before they are capable of reacting by the production of antibodies. Why exposure of embryonic cells to antigen should inhibit the subsequent formation of the specific antibody by these cells, whereas exposure for the first time of adult cells to the same antigen induces the formation of the specific antibody, is another problem that awaits solution. The solution of these problems may be expected to give us a glimpse, at least, of the fundamental cell processes involved in the fabrication of specific proteins. All these phenomena may depend on the 'recognition' of the molecular form of proteins by the cells, a property which might prove to be of wide significance in embryonic differentiation. Immunological methods open the way for experimental approaches to problems of vertebrate embryology that hold promise of great advances in our understanding of the development of function; of physiogenesis rather than morphogenesis.

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GEOGRAPHY AND ITS HISTORY

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THE long history of our section, so admirably summarised and so elegantly expressed by Dr. O. J. R. Howarth at Edinburgh in 1951, reveals that no President has yet ventured to address it on the subject which I have chosen. Some have introduced bits of it as introductory material for a wider theme, and one, Ravenstein, in 1891, gave a masterly account of the development of cartography. I therefore approach it with some hesitation. Nor could I do so without relying heavily on the work of others. If I mention only a few of these I am not unmindful of, or ungrateful to, other and earlier scholars. The work of Dr. Hartshorne, Mr. Crone, Dr. Williamson, Howarth, Mill, Keltie, Heawood, and Beazley, will occur to everyone. And I am in private duty bound to pay particular tribute to Howarth whose untimely death has deprived us all of a wise and benevolent counsellor, and me of a collaborator in the history of geography in Britain in the nineteenth century: no other can take his place. But among British geographers Professor Eva G. R. Taylor must have pre-eminence. No scholar has done as much as she has to recount the past history of our subject, and it has been my privilege to profit from her advice, her written works, and her vast store of unpublished material, over a long period of years. All who are in the least interested in the history of geography must record the debt that they owe to her. It is one of the most melancholy of reflections that her works have been published either at considerable cost to herself or with the aid of subsidies. This indicates either a lack of public interest in the subject, which is unfortunate, or the failure of geographers to give adequate attention to it in schools and universities. If what I have to say produces even a small increase in the interest taken in the history of our predecessors, it will have been justified.

I cannot hope to deal with the whole history of geography. I shall say little about the nineteenth century, and nothing about geography before the year 1500. Even within these limits I must be selective. The subject itself is, however, not inappropriate to this city of Bristol. Robert Thorne the younger was born here, and his theoretical arguments for a passage to Asia across the Polar seas contributed to that long series of expeditions which in the course of centuries made known the whole of the Arctic regions. Richard Hakluyt was admitted to a Prebend's stall in the Cathedral Church of Bristol about 1587. And long before either, merchants of Bristol, steeped in the knowledge of the mythical islands of the Atlantic, had sailed from the port of Bristol to seek for the island of Brazil.

Nor is the time of my address inappropriate, for the middle years of the centuries have been peculiarly productive of great works of geography, including the *Cosmography* of Sebastian Munster, the *Geographia Generalis* of Varenius, the *Essai de géographie physique* of Buache, the *Neue Erdbeschreibung* of Busching, the *Kosmos* of Humboldt, and the last volume of the new *Géographie Universelle*. Each of these works has, in its turn, left its mark on the history of geography.

We have long passed the day when the beginning of modern geography was attributed solely to the work of Humboldt and Ritter. Dr. Hartshorne has even gone so far as to write that, 'It hardly seems too much to say that, had neither of the seamen lived, the development of geography after 1800 would have led

ultimately, even though more slowly, to something like that which we know' (1). But we have yet to assess the precise influence that these two men had on the geography of the nineteenth century, and we have neglected the part played by French geographers. The Geographical Society of Paris was founded in 1821. Its first secretary was P. F. Eugène Cortambert, and one of its founders was the Dane, Malthe-Conrad Brun, who was known in France as Malte-Brun. Cortambert published his *Physiographie* in 1836. In some ways he anticipated the *Kosmos* of Humboldt, for he called his work a general description of Nature : in some ways he anticipated Huxley, who in 1877 thought he had invented the term Physiography for an improved kind of physical geography which he called an introduction to the study of Nature. Malte-Brun's first geographical work appeared in 1803 in collaboration with E. Mentelle, and the first volume of his *Précis de la Géographie Universelle* was published in 1810. This book had a wide circulation, and was well known in England in its original form, in an English translation, and in the abridgement by Balbi. The libraries of Oxford colleges bought it, and it is no exaggeration to say that for many years the work of Malte-Brun was better known in Britain, and more widely read, than any of the works of Humboldt and Ritter. Neither Malte-Brun nor Cortambert is mentioned by Dr. Hartshorne. Nor has any adequate appreciation been made by English writers of the many other French geographers and geologists, who did so much in the late eighteenth and early nineteenth centuries to work out the practical details of natural regions in France. In *The Making of Geography*, by Dickinson and Howarth, is the astonishing statement that no progress in the subject of regional divisions was made from the time of Humboldt until the end of the nineteenth century. Yet the work of these pioneers was given in some detail by Gallois in 1908. I do not suggest that any of these Frenchmen was greater than either Ritter or Humboldt, but I do suggest that they have received insufficient recognition from those who have traced the history of geography in the nineteenth century.

One of the most remarkable facts of the last fifty years is the growth of academic geography in this country, and many of those geographers who are reaping the fruits of that growth display two characteristics—arrogance and despondency. Looking back on the past they thank God that they are not as those so-called geographers were. They know what geography is : their ancestors did not. They go further. Some have recently described themselves as 'the third generation of British geographers.' What is the length of a generation of geographers I do not know, but I suspect that their claim is as statistically inaccurate as it is historically inept. I submit that this is a wholly wrong approach to our subject. It is evident from its history that geographers are always producing what they call a 'new' geography. In our own day we have heard, or read about, the 'new' geography of Mackinder. Yet a study of Mackinder's writings, together with a knowledge of what had gone before, will show that few, if any, of his ideas were original : his originality consisted in his presentation of them. And it is significant that he himself, addressing this Section in 1895, referred in appreciative terms to those veterans of 1869 who had begun the advance in geography. It is also significant that H. R. Mill, one of Mackinder's contemporaries, could say to this Section in 1901 that

> 'we sometimes hear of the New Geography but I think it is more profitable to consider the present position of Geography as the outcome of the thought and labours of an unbroken chain of workers, continuously modified by the growth of knowledge, yet old in aim, old even in the expression of the ideas that we are apt to consider most modern.'

Mackinder, and perhaps Mill, would probably not have approved of much that is now taught in our universities. Nor would they have approved of Pinkerton's *Modern Geography digested on a new plan*, which appeared early in the nineteenth century, nor of the *Précis* of Malte-Brun, which was also on a 'new plan.' Both these geographers, however, tried to be up to date, and Malte-Brun used the latest findings of the German geologists and the latest researches of Humboldt and many others.

Similarly the eighteenth century is filled with 'new' geographies, of which the outstanding were those of Buache and Busching.

P. Bauche had a very wide and long influence. Seeking for a general explanation of the structure of the earth he concluded that mountain ranges coincided with water partings of rivers and that these ranges were continued beneath the sea by submarine ranges marked by emergent features like archipelagoes, islands, and sandbanks (2). His theory of river basins continued to appear in one form and another for more than a century and a half. It was introduced into Germany by Gatterer and into Britain by Pinkerton and by James Pillans, then a master at the Edinburgh High School, discussed in the nineteenth century by most physical geographers of repute, incorporated in regional geographies and in atlases, frequently made use of in examination questions, and adapted to suit diverse needs such as those of the Tennessee Valley Authority, and of others who have put forward similar schemes. Yet it is uncertain how far the idea was original to Buache : it may have been derived from the *Mundus subterraneus* of A. Kircher published in 1665. Kircher formulated a general plan of the mountains of the earth's surface and answered the question why do they apparently end at the end of the land surface by saying 'sed subter Oceani profunditatem eodem ordine continuari'—an explanation almost identical with that of Buache. He also invented an explanation for the greater river systems of the world, bringing them from underground lake sources. Thus the Rhine, Rhone, Danube, and Po all rose from an underground source beneath the Alps, and the great African rivers from a similar source under the mountains of the Moon. But this latter idea may have been derived from reports of Jesuits who had visited Abyssinia. Kircher recorded the opinion of Paez that the two fountains which formed the source of the Blue Nile 'have no issue in the plain on top of the mountain but flow from the root of it,' and Paez himself wrote, 'It is believed here that these springs are the vents of a subterranean lake.' These remarks, and others relating to mountains, may well have furnished the material for what James Bruce described as 'the rashness' with which Kircher advanced the most improbable facts in natural history. If these speculations are correct the theories of Buache must be ante-dated by 125 years.

A. F. Busching, Professor of Philosophy at Göttingen, claimed 'to give an accurate and useful Description of the Earth as far as it is known from the best helps that could be procured on the subject.'

> 'For this end', he said, 'I was under an indispensable necessity of setting about this work as if no system of Geography had been extant before. I am very sensible that there are many treatises of that nature published; however I could not implicitly depend upon, nor safely copy any of them; but was obliged carefully to examine every particular and to have recourse to the first and best sources. My predecessors in this science, indeed, generally copy from each other; and such as have not copied from other systems of Geography have used such helps as are universally known, and open to everyone's perusal, if we except some few particulars. And it is evident that they either had not, or could not have recourse to the best sources; or, which has generally been the case, did not use them with a proper degree of care and impartiality. Hence a person who has the least skill in geography, or knowledge of the Terraqueous Globe, has reason to complain that the systems of Geography hitherto published are of very little service . . .' (3).

Now in so far as this was a claim that his facts were right it was largely justified. And his work attracted much attention. It was translated into French and English. He was described by a contemporary, Baron Riesbeck, as the greatest known geographer in Europe, and, in 1828, by a British geographer as 'the father and founder of scientific geography.' Yet his work was incomplete, in that it dealt only with Europe although he collected some material for other parts of the world. Baron Riesbeck shrewdly observed of the uncompleted parts 'he is deterred by the difficulties of it; and indeed Asia, Africa and America, are not so easy to be described as Europe is' (4). And it must be admitted that in substance there was little new in his description except the facts : the pattern had been set by Sebastian Munster more than 200 years before and had been followed in those great

works, which Busching himself quoted, of Mercator, Ortelius, Hondius, and Jansson, often called Atlases but better described as geographies illustrated by maps, as well as in the innumerable cosmographies which appeared at intervals through the seventeenth and eighteenth centuries. Even the claim to have studied everything afresh had been made before.

Samuel Purchas, whose *Pilgrimage* first appeared in 1613, claimed to ' bring Religion from Paradise to the Arke, and thence follow her round about the world and (for her sake) observe the world it selfe.' This observation he maintained was of use to Geographers.

> ' The studious of Geography may somewhat be helped in that kinde : not that we intend an exact Geographie, in mentioning every Citie with the degrees of Longitude and Latitude but yet limiting every country in his true situation and bounds ; and performing happily more than some which take upon them the title of geographers as their chief profession ; and more than any, which I know, hath done in our language.'

Not long before, Richard Hakluyt, at some date after taking his Bachelor's degree in 1574, gave some lectures, in Oxford or elsewhere, in which he claimed to be ' the first that produced and showed both the old imperfectly composed, and the new lately reformed maps, globes, spheres, and other instruments of this art.' It is difficult to accept his claim to be ' the first.' Nor can the great works of Varenius and of Munster be regarded as wholly original. Some of the ideas of Varenius which modern geographers regard as most revolutionary were borrowed from the German geographer Keckermann, while it is obvious that Munster not only drew heavily on those contemporary scholars whose help he acknowledges but was further indebted to many predecessors. There is no need to pursue this point further. Geographers through the last four centuries at least, have always been dealing with ' the old imperfectly composed ' and ' the new lately reformed ' : therein lies their purpose and their pride. Geography, as its history abundantly reveals, is a subject that is always alive ; without that life it is nothing.

Geographers looking back on the past are often despondent when they read the works of their predecessors. ' Capes and bays ' geography has become a stock joke for after-dinner speakers, whether professional geographers or not, and many have assumed that because geography was badly taught in schools, and hardly mentioned in the universities, there is something unworthy or disreputable about its past. This attitude overlooks two important facts. First, if it is true, which I doubt, that geography counted for little in the past, is not the same true of many subjects which now rank as Sections of this Association ? What geologist would now acknowledge the doctrines of Woodward, or Werner, or Buckland ? What chemist would accept the views of any one before Lavoisier ? And what about the Sections which did not exist a century ago ? This matter has been fully expounded in Howarth's *Retrospect* of the British Association, and I need only remind you that whereas Section E became independent in 1851, Section L was not born until 1901, and Section M until 1912.

The second fact is that contemporaries did not regard geography as of no importance. On the contrary, they regarded it as an essential part of general education. John Milton, not usually regarded as a geographer, criticised the system of education of his day, and in his *Essay of Education* proposed to set up academies throughout the country to be ' at once both school and university.' He included in an early part of his curriculum elementary geography. But his geography went much further than this. Elsewhere he wrote :—

> ' The study of Geography is both profitable and delightfull ; but the Writers thereof, though some of them exact enough in setting down Longitudes and Latitudes, yet in those other relations of Manners, Religion, Government and such like, accounted Geographical, have for the most part miss'd their proportions. Some too brief and deficient satisfy not ; others too voluminous and impertinent cloy and weary out the Reader ; while they tell long Stories of absurd Superstitions, Ceremonies, quaint Habits, and other petty Circumstances little to the purpose. Whereby that which is usefull, and onely worth observation, in such a wood of words, is either overslip't, or soon forgotten : which perhaps brought into the mind of some men, more learned and judicious,

who had not the leisure or purpose to write an entire Geography, yet at least to assay something in the description of one or two Countreys, which might be as a Pattern or Example, to render others more cautious hereafter, who intended the whole work' (5).

He then explained how he came to write his History of Moscovia which, despite its title, he regarded as a work of geography.

John Locke, at the end of the century, like Milton included geography in his educational programme.

'Geography I think should be begun with; for the learning of the figure of the Globe, the situation and boundaries of the four parts of the world, and that of particular kingdoms and countries, being only an exercise of the eyes and memory a child with pleasure will learn and retain them. . . . These things are not all, I confess, that he is to learn upon the Globes. . . .

'With Geography Chronology ought to go hand in hand. . . . Without these two History, which is the great mistress of Prudence and Civic Knowledge, and ought to be the proper study of a Gentleman, or a man of business in the world; without Geography and Chronology, I say, History will be very ill retained and very little useful. . . . 'Tis by these two that the actions of mankind are ranked into their proper places of time and countries, under which circumstances they are not only much easier kept in the memory; but in that natural order are only capable to afford those observations which makes a man the better and abler for reading them' (6).

Dr. Samuel Johnson would certainly not be regarded as a geographer. But he wrote the Introduction to John Newberry's collection of voyages and travels known as *The World Displayed*, and Boswell said of him that 'an inquiry into the state of foreign countries was an object that seems at all times to have interested him.' In his *Dictionary* Johnson quoted in full the definition of geography given some years earlier by Isaac Watts.

'Geography in a strict sense signifies the knowledge of the circles of the earth's globe, and the situation of the various parts of the earth. When it is taken in a little larger sense it includes the knowledge of the seas also; and in the largest sense of all it extends to the various customs, habits and governments of nations.'

Johnson's own views were expressed in his essay on Education:

'The necessity of some acquaintances with geography and astronomy will not be disputed. If the pupil is born to the ease of a large fortune no part of learning is more necessary to him than the knowledge of the situation of nations on which their interests greatly depend; if he is dedicated to any of the learned professions it is scarcely possible that he will not be obliged to apply himself in some part of his life to these studies, as no other branch of literature can be fully comprehended without them; if he is designed for the arts of commerce or agriculture, some general acquaintance with those sciences will be found extremely useful to him; in a word no studies afford more extensive, more wonderful, more pleasing scenes; and therefore there can be no ideas impressed upon the soul which can more conduce to its future entertainment.'

He went on to urge that the 'nakedness of science' should be decorated by introducing narratives.

'Thus in explaining the state of the Polar regions it might be fit to read the narrative of the Englishmen that wintered in Greenland which will make young minds sufficiently curious after the cause of such a length of night and intenseness of cold. . . . When they have read this treatise it will be proper to recommend to them Varenius's *Geography* and Gregory's *Astronomy*' (7).

Scattered throughout the pages of Boswell's *Life* are many references to the interest of the greater man in geography. Some of these are not particularly flattering to the subject, but as Boswell said, 'he could talk on any sides of a question.' Thus while he was 'much pleased with the conscientious accuracy of that celebrated navigator' James Cook, he had some acid remarks to make about the voyage and its results. But he had an eye for country, as evidenced in his account of his journey to the Hebrides; he had read, as an undergraduate at Pembroke College, Oxford, Lobo's *Voyage to Abyssinia*, and at the age of twenty-six he published a translation of it; he wrote a preface to Macbean's *System of Ancient Geography* in 1749; and on his journey to Harwich in 1763 'he had in his pocket *Pomponious Mela de situ Orbis* in which he read occasionally, and seemed very intent upon ancient geography.'

From Samuel Johnson it is fitting to turn to Edward Gibbon the historian. Pinkerton said of him: 'Why did he not write geography! Why has a Strabo been denied to modern times!' And he

recorded that his collection of voyages and travels formed the most chosen part of his library. Lord Sheffield, his closest friend and the editor of his autobiography and his Miscellaneous Works, noted that

> 'his attention to geography had always been very great, and few were better informed in that science. His friend Major Rennell was of that opinion, and I cannot cite a higher authority.'

Gibbon himself had already written of the 'perspicuity such as made Bentley a critic and Rennell a geographer.' He wrote a geographical account of ancient Italy, made use of wide geographical reading in his *Decline and Fall of the Roman Empire*, left behind him many papers on geography, and in the remarkable abstract of books that he read showed clearly that while he was, naturally and properly, mainly interested in classical geography he was none the less familiar with modern developments of the subject. He knew the works of Busching and D'Anville: he could comment on the prospects of the discovery of the north-west passage, discuss the effects of the eruptions of Vesuvius, and appreciate the new world of Siberia revealed by the journey of J. G. Gmelin. He became a member of the African Association, the first geographical society in Britain. And it is surely of some significance that in that Association men of such diverse interests should meet for the common purpose of promoting the knowledge of geography.

Mr. G. R. Crone and Mr. R. A. Skelton have suggested that the great collections of travels which appeared in England in the eighteenth century were manifestations of contemporary taste in 'an age of expansion in the arts of living' (8). I would carry the suggestion further: the appreciation of geography as part of the culture of an educated man was surely indicative of this 'expansion in the arts of living.'

Limitations of my own knowledge as well as of the time at my disposal have restricted this survey to four Englishmen in the seventeenth and eighteenth centuries. But I have little doubt that what was true of England was true of other countries. The works of the French Political Scientists from Bodin in the late sixteenth century to Rousseau, of great naturalists like Buffon, of the historian de Brosse, if examined closely, show many parallels with their counterparts in England. And there is reason to think that the same principles apply elsewhere.

Not only do we find geography appreciated and understood by educated men, but it was introduced into their writings in such a way that they must have assumed that their readers would understand what they wrote. In his brilliant Essay in the Maclehose edition of Hakluyt's Voyages in 1905, Walter Raleigh devoted one section to 'the influence of the Voyages on Poetry and Imagination.' In the course of this he writes:

> 'Without the Voyagers Marlowe is inconceivable. His imagination is wholly preoccupied with the marvels of the world and his heart possessed by the new-found lust of power.'

Marlowe put into the mouth of the dying Tamburlaine the words:—

> 'Give me a map; then let me see how much
> Is left for one to conquer all the world.'

He then proceeds to describe the map. Some critics have accused him of inaccuracy because his account does not agree with modern geographical knowledge, but Ethel Seaton (9) has conclusively shown that Marlowe knew his geography, and made extensive use of contemporary maps particularly those of Ortelius. His audience would not have accused him of inaccuracy.

Milton's poems are full of geographical references, and many of his lines mean little or nothing without a knowledge of geography as understood in his day. Who without some such knowledge can understand these?

> 'As when two polar winds, blowing adverse,
> Upon the Cronian Sea, together drive
> Mountains of ice, that stop the imagined way
> Beyond Petsora eastward to the rich
> Cathaian coast.'

We must go to Cluverius or some other geographer of the period to discover that Milton selected one of the several alternative names for the Hyperborean sea, and we must know something of the proposals for a north-east passage to understand what was 'the imagined way' to 'the

rich Cathaian coast.' Or again in *Paradise Regained* we read :

' Syene, and where the shadow both way falls
Meroe Nilotic isle, and more to west
The realm of Bocchus to the Black-moor sea
From th' Asian Kings and Parthian among these
From India and the golden Chersonese
And utmost Indian isle Taprobane.'

Here so much geography, theoretical and regional, is crowded into six lines that no one but an accomplished geographer could follow the description. Once again Cluverius and his contemporaries can easily provide the answer.

One last example must suffice. The poet Shelley had a wide geographical knowledge. His poem *Mont Blanc*, written in 1816, is too long and too well known for quotation, but at the very time he was writing it he was also sending a prose description to his friend Peacock. This letter contains a vivid description of Alpine glaciers :—

' These glaciers flow perpetually into the valley, ravaging in their slow but irresistible progress the pastures and the forests which surround them, performing a work of desolation in ages, which a river of lava might accomplish in an hour, but far more irretrievably ; for where the ice has once descended, the hardiest of plant refuses to grow ; if even, as in some extraordinary instances, it should recede after its progress has once commenced. The glaciers perpetually move onward, at the rate of a foot each day, with a motion that commences at the spot where, on the boundaries of perpetual congelation, they are produced by the freezing of the waters which arise from the partial melting of the eternal snows. They drag with them, from the regions whence they derive their origin, all the ruins of the mountain, enormous rocks, and immense accumulations of sand and stones. These are driven onwards by the irresistible stream of solid ice ; and when they arrive at a declivity of the mountain, sufficiently rapid, roll down, scattering ruin. I saw one of these rocks which had descended in the spring, (winter here is the season of silence and safety,) which measured forty feet in every direction.'

He ends with a discussion of the advance and retreat of glaciers. He had read Buffon and Saussure and disputed the latter's theory of periods of ' increase and decay.' He preferred the opinion of ' the people of the country ' and concluded :

' If the snow which produced this glacier must augment, and the heat of the valley is no obstacle to the perpetual existence of such masses of ice as have already descended into it, the consequence is obvious ; the glaciers must augment and will subsist, at least until they have overflowed this vale.'

To all students of literature it is well known that the natural features of the earth have provided inspiration for poets and painters and scenes for descriptive writers. This point was dealt with at length, and long ago, by Humboldt. Since then W. Sharp has written his *Literary Geography*, a book not quite living up to its title, and dealing only with British writers. More recently some geographers have written on Defoe, Lamb, and Hardy. I will add only one further example. What geographer of the eighteenth century, given fifteen short lines, could better describe the Netherlands than did Oliver Goldsmith ?

' To men of other minds my fancy flies,
Embosom'd in the deep where Holland lies,
Methinks her patient sons before me stand,
Where the broad ocean leans against the land,
And, sedulous to stop the coming tide,
Lift the tall rampire's artificial pride.
Onward, methinks, and diligently slow,
The firm connected bulwark seems to grow ;
Spreads its long arms amidst the wat'ry roar,
Scoops out an empire, and usurps the shore.
While the pent ocean, rising o'er the pile,
Sees an amphibious world beneath him smile :
The slow canal, the yellow-blossom'd vale,
The willow-tufted bank, the gliding sail.
The crowded mart, the cultivated plain,—
A new creation rescu'd from his reign.'

I turn now to a different point. One of the most important features of geography in our own day is its ability to help in practical affairs. In land utilisation survey, in urban geography, in problems of colonial development, and in the conservation of resources, to name a few examples, it has something to contribute. This is no new feature. Geography has always had a practical side. In the sixteenth and seventeenth centuries theoretical geography not only contributed to practical exploration by discussing matters such as the sailing route across the Polar seas, but went further and was closely allied to navigation. Hakluyt advocated the establishment of a Lecturer on Navigation ' a matter of great consequence and

importance for the saving of men's lives and goods.' Hakluyt was unsuccessful but his idea was taken up by Edward Wright and by Sir William Monson. And in 1597 Sir Thomas Gresham's College was founded and the Professor of Astronomy was required to lecture on geography and the art of navigation. In 1619 Sir Henry Savile founded the chairs of Astronomy and Geometry at Oxford, and the Professor of Astronomy was particularly charged with the teaching of geography and of navigation in relation to mathematics. Savilian Professors continued to do their duty throughout the seventeenth and eighteenth centuries. And it may be added that Norie's *New and Complete epitome of Practical Navigation* which was dedicated to the Directors of the East India Company, and reached a sixth edition in 1819, contained a section on geography. The development of this side of geography has recently been brilliantly recorded by Professor Taylor in *The Mathematical Practitioners of Tudor and Stuart England.*

In 1587 Hakluyt referred to geography as the ' eye of history ' and from the context it is clear that by geography he meant maps. And probably the same meaning should be attached to his better known phrase, geography and chronology were as the sun and the moon, ' the right eye and the left of all history.' Here again is a practical application of geography in a narrow sense. In a wider sense this is seen in the close connection between classical studies and geography. Annotated editions of the works of classical geographers, and bulky geographical dictionaries are a common feature. Nor would one expect anything different. In the centuries in which classical studies played such a prominent part it would be unreasonable to do so ; but the fact that these works were produced and widely read is an indication of the status of geography. If modern geographers can make similar contributions to the general culture of our age they will, in that way alone, justify their existence.

Finally in the wider sphere of life there was a practical aim. Some idea of this was given by Johnson in a passage already quoted. Locke declared that without a knowledge of geography gentlemen could not understand a newspaper although he doubted their willingness to read the lengthy works of Heylyn and Moll. Almost every geographer claimed that geography was a necessity for statesmen, clergymen, business men, and indeed for all who had to deal with the realities of life. This was no idle boast.

> ' The head of the first ministry of King George I more than once declared emphatically that if the Baltic convoy miscarried it would not be possible to fit out men of war or merchantmen for the next year's service ' (10).

The reason was obvious. He knew that the geographical resources of that area—pine timber, pitch and tar, hemp and flax—could not be obtained at that time in sufficient quantity from any other source. Conversely, Samuel Pepys, who was educated at St. Paul's and at Cambridge, discovered when he became an official a defect in his education. ' I do perceive,' he wrote in his diary, ' that I am very short in my business through not knowing the geographical part of my business.' And Clarendon, the Prime Minister of Charles II, educated at Oxford, was equally deficient, and when he had to take in hand the preparations for the defence of the Thames against the Dutch fleet admitted complete ignorance of the coast and the river.

Two geographers well known to all of us have recently written : ' Geography begins only when geographers begin writing it.' I am not sure what the real implication of this statement is, but the history of geography seems to me to provide some indication. It is quite clear that although for long periods there has been geography, for equally long periods there have been no geographers in the technical sense of the word. Sebastian Munster was a Professor of Hebrew. His *Cosmography* consisted of six books, the first of which might be called mathematical and general geography, the next three were devoted to Europe, and the remaining two to Asia and Africa. I would estimate that 80 per cent. or perhaps more of the book would now be described as history. And it is fair to say that he wrote it for the benefit of historians. But even before he wrote, Peter Apianus had produced another, and very much shorter, *Cosmography*, the bulk of which was mathematical and general with a very brief sketch of what,

to borrow a modern term, could be called ' regional ' geography. In other words there was no accepted meaning for the term ' Cosmography.' One could pursue this argument into the seventeenth century. The geographies of Cluverius, of Varenius, and of Nathanael Carpenter followed no recognised plan, and each differed from the other in important respects. Moreover, side by side, the great cosmographies went on until in the eighteenth century the term cosmography was replaced by geography. In that century the word ' grammar ' was frequently added so that we have the well-known geographical grammars of Gordon, Salmon and Guthrie, side by side with the geography of Busching, Bankes, Middleton, Pinkerton and Kelly. These latter works were often very bulky, but they at least adorned the shelves of many gentlemen, even if they remained unread. Few of their writers were geographers as we understand the term.

Nor was geography confined to those who wrote cosmographies and geographies. Richard Rawlinson, summarily described in the Dictionary of National Biography as ' topographer and nonjuring bishop,' produced his *New Method of studying History, Geography and Chronology* in 1730. This was an English translation from the French of L. du Fresnoy ' with a variety of improvements and corrections.' It is in no sense a geography, and as may be judged from its title it deals mainly with history. But it contains a chapter on the study of geography, and a list of works, historical and geographical, with frequent comments on their worth. Geography, as defined by Rawlinson, was one of the foundations of history, and in this respect he differed a little from Hakluyt in that he had a somewhat wider conception of the subject. It is, however, from his bibliography that one can get a better idea of his views. Under the heading of Geography are the works of Cluverius and Varenius, together with those of Ortelius and Blaeu, and many other contemporary geographers as well as the editors of classical geography. On the other hand, Heylyn's *Cosmography* appears under the heading of Chronology and Universal History, while many books with obvious geographical titles are found under regional headings where they are included as history. We know from his other writings that Rawlinson had a wide interest in geography, but what did it mean to him? It was a necessary preliminary to history, and, as often as not, part of history itself. Is it mere fancy to suppose that he might have been accused by some contemporary ' geographer ' of ' taking the " Cosmos " out of Cosmography ' ? (11).

A. F. Tytler, Professor of History at Edinburgh at the end of the eighteenth century, published his lectures in 1801 under the title *Elements of General History, ancient and modern, to which are added a table of chronology and a comparative view of ancient and modern geography*. This comparative view was inserted at the end of his first volume, and consisted of two tables, headed ' Modern Europe ' and ' Ancient Europe,' or ' Modern Asia ' and ' Ancient Asia ' and so on. The tables conveyed little information, but they were accompanied by maps based on those of D'Anville. Tytler's volumes dealt with history up to 1715 and for this period he probably considered it was adequate to give his brief lists of names and his maps. In his summary account of the progress of science and literature in Europe from the end of the fifteenth to the end of the seventeenth century geography is not mentioned. Tytler's work was continued, to the year 1820, in a third volume, by E. Nares, Regius Professor of Modern History at Oxford, and he added a section on the ' State of Arts, Science, Religion, Laws, Government etc.' He dealt with the development of botany, electricity, mineralogy and geology, geography, and other subjects during the eighteenth century, and he included geography among the sciences which had made ' very extraordinary progress.' It is of interest to find that Nares in 1824 relied on French geographers, although he was familiar with the work of Humboldt. The development from the work of Tytler is of some significance. Nares dealt with a new age and recognised its progress, so that his corresponding chapter is fuller, and here modern geography takes its proper place. Yet almost at the same time John Horsley, who was a Fellow of the Royal Society, was contributing a map of Roman Britain to the English

translation of D'Anville's *Compendium of Ancient Geography*. The translator, considering 'that the student of ancient geography should have distinct ideas of the ancient inhabitants of Europe' borrowed his account of these people largely from the then modern geography of Pinkerton. All this leads to the conclusion that there have been many kinds of geography and many writers of geography, and that present day geographers should be modest and circumspect in their claims. Who is to say what is and what is not geography, or who is or who is not a geographer? In 1577 Richard Willes attempted a definition.

'Who but geographers doe teach us what parts of the earth be colde, warme, or temperate? Of whom doe we learn how to divyde the world into partes, the partes into provinces, the provinces into shires? Of Geographers. Unto whom have wee to make recourse for Mappes, Globes, Tables and Cardes wherein the dyvers countreys of the world are set down? unto Geographers. Set Geography asyde, and you shall neither be able to get intelligences of the situation and strength of any citie, nor of the limits and boundes of any country, nor of the rule and government of any kingdome, nor be able to wel travayle out of your owne doores. Wil you see what wise and experte travaylers, skilful in geometry and astronomye (for that is to be a Geographer in deede) be able to doe? . . . The great commodities our nation reapeth by the travayle of our countrymen . . . wil be sufficient testimony unto all us Englishmen, what it is to be a skilful travayler, what to be a paineful Geographer, and learned' (12).

With little difficulty this can be translated into the language of our day—climatology, cartography, regional and political geography, exploration, mathematical geography, and economic geography. And, as doubtless Willes would have asserted if challenged, there may be other subjects. In our own day we have wasted much time in discussing what geography is, or even whether there is such a subject. Some of us come near to the poet who asked of the cuckoo if he called it a bird or but a wandering voice—though we would use the less poetical language, 'a point of view.' If, when we say that 'geography begins when geographers begin writing it' we mean that there may be many geographies and many kinds of geographers we are on sound historical ground. If we mean that only those are geographers who conform to our own particular ideas of geography, we are unreasonably assertive in our belief and unjustifiably arrogant in our assertion of it. Geography has in the past had room for all who are prepared to contribute to it.

Nor need we pursue too closely the definition of geography. I have quoted the definition of Watts; there was a strict sense, a little larger sense, and the largest sense of all. Watts himself indicated what he meant by writing *The First Principles of Astronomy and Geography*, to cover the strict sense, and recommending other books including 'that excellent manual *Gordon's Geographical Grammar*' and the larger *Geography Rectified* by Morden and *Geography* by Moll. Here are wide limits, and much diversity. And the history of geography shows that this has always been so. Why should we, a mere generation in the centuries of geographers, take upon ourselves to limit the definition of our subject? I would quote the words of my immediate predecessor:

'Whether our inclination is to the physical or to the human side does not matter—there is work for all, and what is of extreme importance, work for exponents of different subjects.'

This is precisely what the history of geography teaches.

Nor need we be worried about controversies within our subject. In the seventeenth century there was much argument as to what geography meant, how it was related to cosmography, whether chorography was or was not geography, and whether geography should be divided into general and special, or external and internal. The geographers of the day had their fun and no one was any the better—or worse—off. We do the same, but in the long story of our subject it does not really matter very much. We have recently had an outburst of argument about determinism and possibilism, and many seem to think that this is a problem which first thrust itself on geographers with the writings of Buckle and Ratzel. In fact, it is a problem as old as geography itself, and will remain a problem so long as man continues to

exercise his powers of thought and argument.

The history of geography is long and honourable. No geographer need apologise for it or be ashamed of it. Professor Taylor wrote in the opening sentence of her preface to *Tudor Geography*, 'The History of Geographical Thought has yet to be written.' Some advance has been made since she wrote those words in 1930, but there is still almost unlimited scope for research. The rewards will be great because it is only when the geography of our day is seen against the background of its history that its present position can be appreciated and its future prospects assessed.

It is no uncommon practice for geographers in the past to explain or excuse their own shortcomings, and I may be allowed to quote two of them in order to excuse myself. Richard Willes, who discussed the theoretical arguments to which the first voyage of Martin Frobisher gave rise, for the benefit of the Countess of Warwick, ended thus :

'If not so skilfully as I would, and was desirous fully to do, at least as I could and leisure suffered me, for the little knowledge God hath lent me, if it be any at all, in Cosmographie and Philosophie, and the small experience I have in traville' (13).

This graceful ending, omitted by Hakluyt when he reprinted Wille's *Discourse*, came naturally from one who was a poet, and for a time a Professor of Rhetoric at Perugia. As a traveller his experience was limited to a few countries of Europe so that, although small, it was at least greater than that of Robert Burton who 'never travelled but in map or Card.' But it was much less extensive than that of William Dampier, described by Dr. Williamson as 'a born scientific observer.' Dampier ended his *Discourse of the trade-winds, breezes, storm, seasons of the year, tides and currents of the torrid zones throughout the world*, with words that may well end my own address :

'And thus I have finished what my own Experience, or the Relations of my Friends have furnished me with on this useful subject . . . which I humbly offer, not as a compleat and perfect account, but as a rude and imperfect Beginning or Specimen of what may be better done by abler Hands hereafter. And I hope this may be useful so far as to give a few Hints to direct the more accurate Observations of others (14).

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THE ECONOMIC FOUNDATION OF WAGE POLICY

Address by

PROF. J. R. HICKS

PRESIDENT OF SECTION F

TWENTY-SIX years ago, when the British Association was meeting at Cape Town, the President of Section F was Henry Clay, and he took as his subject 'The Public Regulation of Wages in Great Britain.' I remember, from that time, how great a stimulus that paper was to my own thinking, and I have not changed my opinion of it. Of the problem of wages, as it then stood, Clay gave what I still believe to have been a substantially correct analysis. But a tide of events, which set in almost as he spoke, was to bring about a great transformation. The revolutions which have occurred in the realm of fact, quite as much as those which have occurred in the realm of ideas, have presented us with a wages question which looks very different from the wages question of 1929. Nevertheless, underlying the differences, some of them real differences, some of them seeming, there is a certain continuity. I propose, in this paper, to survey some aspects of the developments which have occurred, and to attempt an estimation of their significance. I believe that the time has come when it is desirable to make an effort to bring the whole matter back into focus.

The first thing on which Clay had to remark, from the standpoint of 1929, was the spread of collective bargaining (and other forms of *public regulation*) from the mere fraction of the wage-earning population (perhaps a quarter) which had been covered some twenty years previously, to a point where (as he said) 'we may safely conclude that there are few important gaps left in the provision for the settlement of wages by collective bargaining in Great Britain.' But then he went on:

> 'The precise nature of this change is worth some consideration. It was not the introduction for the first time of standardised rates of pay in time-work occupations. Even if we leave out of account the considerable part of the field covered by Trade Unionism at the beginning of the century, it is probable that in most districts, in which an occupation was followed by considerable numbers, there were customary rates commonly recognised, which the majority of employers observed. These rates were not so definite and secure as they became when they were embodied in a collective agreement; but outside the so-called "sweated trades," they were a limitation on the freedom of the individual employer to vary rates. Immediately, wages were fixed for him rather than by him, although ultimately they had to conform to the demand for labour, of which he was the channel. Nor was the change a universal substitution of collective for individual bargaining about rates. In piece-work industries after the change, as before, the vast majority of rates were settled by an individual bargain between the workman and the employer's representative. . . . The significant and essential change was the change in procedure. Wage-rates in any case have to be adjusted to changes in the demands for different kinds of labour, changes in the purchasing power of money, changes in the general prosperity and activity of industry. Before the war, outside the organised industries, the adjustment was made by the individual action of the employers, who first felt the need; to-day the process of general wage-changes has, we may say, been constitutionalised.'

I have quoted this passage from Clay's paper because it seems to me to describe, with considerable precision, just what it was that had happened. It has never been the general rule that wage-rates have been determined simply and solely by supply and demand. Even on pure grounds of efficiency, it is desirable that the wage which is offered should be acceptable, acceptable to both the worker himself, and to those with whom he is to work. There has in consequence always been room for wages to be influenced by non-economic forces—whether by custom (which, economically speaking, means supply and demand of the day before yesterday), or by any other principle which affects what the parties to the wage-bargain think to be *just* or *right*. Economic forces do affect wages, but only when they are strong enough to overcome these *social* forces. Now what happened, as a result of the spread of public regulation, was that the social forces grew in strength. As Clay put it,

> 'The mere fact of publicity, or organised discussion, invites appeal to social and ethical standards of "fair" and "living" wages, to pseudo-principles such as the sanctity of pre-war *real* wages, to the unpopularity of reducing the rates of wages of the lower paid workers, none of which have any bearing on the capacity of industry to pay wages and provide employment.'

The main thing is that the new procedure insulated the wage structure from its old exposure to economic pressures, so that it could move—to a considerable extent—on a course of its own.

But the economic forces were not abolished by the increased resistance which could be offered to them; if they were not allowed to influence the determination of wages, they would express themselves in other ways. If the actual wage-level drew too far away from the 'equilibrium' wage-level, being (as, in fact, it would be) above the equilibrium wage-level, the discrepancy would express itself in a growth of unemployment. And though the 'social' wage-structure could stand up to a certain amount of unemployment, there must (so Clay thought) be a limit beyond which it must give. Then, after much pain and grief, there would be a return to something nearer to an 'equilibrium wage'.

That is what Clay expected to happen; but, as we know, it is not what did happen. Unemployment did increase, as he expected; but though the wage-structure yielded a little, on the whole it did not give way. What did give was another link in the chain—the Gold Standard.

In the new world which began after 1931 the problem of wages is bound to have a distinctly different character from that which it had in the older time. Since 1931, wages questions have been closely associated with monetary questions; it is even true that the *general* level of wages has become a monetary question. So long as wages were being determined within a *given* monetary framework, there was some sense in saying that there was an 'equilibrium wage,' a wage that was in line with the monetary conditions that were laid down from outside. But the world we now live in is one in which the monetary system has become relatively elastic, so that it can accommodate itself to changes in wages, rather than the other way about. Instead of actual wages having to adjust themselves to an equilibrium level, monetary policy adjusts the equilibrium level of money wages so as to make it conform to the actual level. It is hardly an exaggeration to say that instead of being on a Gold Standard, we are on a Labour Standard.

I should like to make it quite clear, before going further, that I myself regard the Labour Standard as an unquestionable benefit. So long as we retain it, we are protected against the unemployment that arises from a discrepancy between the actual wage-level and the 'equilibrium wage-level'; and though there are other causes of unemployment than this, it has been one of the main causes of unemployment in the past, and it is a great gain that this cause of unemployment has been removed. It is not a gain which anyone should desire to give up; it is, nevertheless, not a simple matter to find ways by which we can be sure of preserving it.

For the Labour Standard, in our twenty years' experience of it, has been shown to possess a number of defects, of which some are decidedly dangerous. The first set of

defects, which merit attention, though they fall rather outside my present subject, relate to the international consequences of the new arrangements. If the value of the pound is fixed in terms of British labour, and the value of the mark is fixed in terms of German labour, the pound–mark exchange will not remain steady unless the value of British labour in terms of German labour remains steady ; and this last is a matter of the real economic progress and economic policies of the two countries ; it requires something more than a monetary arrangement to control it. While the Gold Standard was an international standard, the Labour Standard is a national standard ; even at its best, it is bound to be a source of difficulty in the sphere of international currency relations. But these difficulties are very familiar ; I need do no more than allude to them in this place.

Already in the first years of the Labour Standard, these international repercussions were a grave preoccupation ; indeed, at that time they were the only consequence of the change which aroused much anxiety. But as time has gone on another weakness has been revealed, which (at least for the time being) has pushed the international weakness into the background. Whereas, under the Gold Standard, the value of the national money in terms of the Standard was fixed by the State (and the maintenance, or in a few cases the regulation, of this gold value was recognised as a major responsibility of the State), under the Labour Standard the value of money in terms of labour undergoes no deliberate determination. It is a mere by-product of the process of wage-fixing ; wage-fixing being sectional, the value of money in terms of the Standard comes to be determined by impersonal forces, and those forces seem to have a strong tendency to pull it one way.

It is not surprising that this danger took some time to declare itself. In the last days of the old system, when the new (fortified) wage structure was resisting the monetary pressures that were being exerted upon it, successful resistance expressed itself in the form of rigidity. Economic forces were pressing wages downwards ; social forces held them up. The net effect was that the level of money wages held firm. It was very natural for Clay, in 1929, to think in terms of rigid wages. Over the years 1924 to 1928 (the only ' normal ' years which were available to him as a basis for generalisation) the Bowley index of weekly wage-rates in Great Britain had been rocklike—it had never moved by as much as a point. Under the impact of the Depression, there was, indeed, a marked *fall* in wages (though much less than the fall in the cost of living) ; but it was some time before the relief of that pressure led to a rise which went beyond the previous level. As late as 1937, the wage-index was within one per cent. of its 1924–8 figure. Thus, the Labour Standard started operations with the very natural assumption that the level of wages (or the value of money in terms of labour) could, at least in Britain, be taken as given. With rigid wages, the problem of the new Standard was one of external stability ; internal stability did not seem to be a problem.

During the war years the position was, of course, very different ; but war inflation is no new experience, and the significance of war inflation for the future of the Labour Standard did not reveal itself at once. It is only in the years since 1945, which have been marked by a continual rise in the wage-index, rarely falling short of 4 per cent. per annum, that the existence of a special problem of the Labour Standard has become apparent. Must we conclude, from the experience of the last ten years, that the Labour Standard has an inherent inflationary bias ? If so, is it much more dangerous than the similar biases which have been detected in other monetary systems ? These are the main questions which I want to discuss in the rest of this paper.

One further point must be cleared out of the way before I can begin. It is possible to argue that the main evil of monetary instability, as it was known in past times, was the unemployment which periodically accompanied it ; if we have to expect an age of monetary instability, but are guaranteed against the accompanying unemployment, the expectation can be borne with equanimity. There is something in this argument, but the optimism which it engenders can be carried too far. A rapid rate of wage-inflation must surely

be an evil, if only because of the diversion of effort into fixing and re-fixing of wages and hitherto stable prices, effort which might otherwise be used for more productive purposes. A very rapid rate of wage-inflation could threaten the continuance of the Labour Standard altogether. I would, however, admit that there is some rate of wage-inflation which, if it can be prevented from accelerating, is by no means intolerable ; the question is what means exist for keeping it within tolerable limits.

Let us then go back to the labour market, and look at it more closely. Granted the Labour Standard, can we lay down any general principles about the ways in which wages are likely to be determined ? What are the factors making for inflation, and what (if any) are those by which the inflation may be held in check ? These are questions which can, I believe, be answered if we tackle them in the spirit of Clay's analysis, though we get no sensible answers to them if we persist in treating them as pure problems of economics, in a narrow sense.

The purely economic analysis of collective bargaining, in terms of the theory of bilateral monopoly, used to conceive of wage-rates being fixed, on a balance of bargaining advantage, between a trade union which sought to push wages as high as possible without causing intolerable unemployment, and an employers' association which sought to press wages as low as possible without causing a shortage of labour. If one thinks in those terms the prospects of anything like stable wages, under a Labour Standard, do indeed look bleak. For the labour market then becomes a pure case of unstable equilibrium. It is not necessary to suppose that each trade union is completely safeguarded against the danger of unemployment in its own trade. If it is laid down that a rise in wages in the A-industry is not to produce any general increase in unemployment, that is enough. For effective demand (for goods in general) will then have to be expanded somewhat when A-wages rise.[1] But that will increase the bargaining power of the workers in other industries, so that if wages in these other industries are determined on the same bilateral monopoly principle, they will rise, too. But that, under the assumptions we are making, will call forth a further increase in effective demand, which provides an opportunity for a further rise in the A-industry. The only thing which would slow down the general rise in wages would be the time taken to negotiate wage-changes ; and that, if it was to people's interest to speed it up, could presumably be speeded up.

Now I would quite admit that there are some occasions on which the actual process of wage-fixing takes a form which is not far away from that implied in this model ; but I find it hard to believe that it has any general validity. We are now learning that the pure theory of monopoly has a very limited application to the behaviour of business men on the markets on which they sell their products ; it is even less likely that it has much application to the labour market—on either side. We get a better clue to actual behaviour if we think of wages as being determined by an interplay between social and economic factors, instead of being based on economic factors —and crude economic factors at that—alone. As we have seen, this was true to a considerable extent under the old conditions. It is still more likely to be true under the Labour Standard, when everyone is bound to have some realisation that the number of shillings in the pay packet are not the thing that really matters. Money wages are not demanded for their own sake, but as a means towards something else.

The only exception to this rule which I can think of, and it is a very partial exception, is that a wage claim may sometimes be made on the basis of comparison with what the wage of that class of labour has been in the past. It certainly was true, in the old days, that a wage-cut would be followed, at the first opportunity, by a demand for a restoration of the wage to its former level. But with the increased

[1] How far this rise in demand comes about *naturally* (if monetary and fiscal policy does not prevent it), or how far deliberate intervention is needed to procure it, is a question that I do not have to raise in this paper. It will be simpler to talk as if ' steps have to be taken ' to procure the increase in demand ; but by using such expressions I do not mean to prejudge the question whether these steps are of a positive or negative character.

strength of the unions, under the Labour Standard, all that is left of this point is the general resistance to any cuts in previously agreed wage-rates, which is one of the principles on which the new wage-system seems to proceed. It is true that if wages can go up, but cannot go down, that is enough in itself to give the system an inflationary bias ; it does not, however, follow that the bias will be so strong as to be unmanageable.

Otherwise, if we think of the grounds on which cases are made for a rise in the wages of any section of labour, the mere money value of the wages received has nothing to do with them. Wage disputes are not concerned with money wages ; they are concerned with the relations of money wages to the money values of other things. But the things with which comparison is made differ from one wage-dispute to another ; it will be useful to classify them under three heads. Of course, it may often happen that more than one sort of comparison is playing a part in some particular claim ; nevertheless, the distinction between them is readily recognisable in practice. And the economic significance of claims on one ground or another is decidedly different.

The three things with which comparison is most naturally made are (1) the prices of the things labour buys—the cost of living, (2) the wages of other workers—differentials, (3) the profits earned by the employer. I propose to discuss these three comparisons rather separately, but in each case from a double point of view. On the one hand I want to ask about the conditions in which wage-claims on the basis of each comparison are most likely to be pressed—a question which, if we can answer it, may throw some light upon the probable extent of wage-inflation in the future ; on the other hand, I want to say something about the attitude which other members of society should take towards wage-claims made on one basis or another, with special reference to the economic arguments by which that attitude may be (or perhaps I may dare to say should be) affected.

The first of my three heads is the cost of living. The ratio of wages to the cost of living is what the economist calls real wages ; the desirability of having real wages as high as possible, consistently with high employment, is a social objective which he is inclined to take for granted. Nevertheless, it is clear that wage-claims are not ordinarily made on the mere ground that the workers would like to have higher real wages ; rises in real wages do for the most part come about in fact as a consequence of rises in productivity (either at home or abroad) without wage policy playing, at bottom, any more than a passive part in the process. When the cost-of-living motive does become important, in relation to wage-claims, is when extraneous factors are tending to produce a rise in the cost of living relatively to wages—or what comes to the same thing, a fall in real wages. It is in such circumstances that the cost-of-living motive takes the lead, and becomes, under the Labour Standard, an independent force making for inflation.

But in a modern economy, what has normally to be expected is rising productivity ; and rising productivity, combined with constant money wages, means falling prices. So long as this tendency remains dominant, there is no need for there to be any pressure for rising money wages on account of the cost of living. The cost of living appears as an independent cause of wage inflation only at times when the normal course of economic progress is interrupted ; its appearance is a symptom that something more fundamental is going, or has gone, wrong. Thus it is, of course, a major issue in war-time ; it is also important whenever the march of rising real wages suffers a setback, due (for instance) to some change, adverse to this country, in our economic relations with the world outside. The novelty of the Labour Standard is that in the latter case as well as the former, the cost-of-living motive becomes a source of inflationary pressure. In this respect the lessons of the Korean episode were, perhaps, more significant than those of the war with Hitler.

The anti-inflationary measures which are available, under the Labour Standard, to deal with such situations are price-control and, if necessary, rationing ; it is one of the costs of the Labour Standard that these measures require to be introduced in order to deal with slighter and more temporary falls in real wages than

would previously have required such drastic treatment. Further, though this is the only way out, it is difficult to make it wholly effective ; if it is not wholly effective, it brings other troubles with it.

Suppose, for instance, that some external event (say an adverse movement of the terms of trade) makes it necessary to reduce consumption by a small percentage. If the reduction could take place by a rise in prices, without wages rising, prices would fall again when the shortage was over, and consumption would recover without further action. Alternatively, if the rise in wages could be wholly prevented by control over prices, the reduction in consumption (which cannot be avoided) would be brought about by an excess of demand over supply ; supplies would recover when the shortage was over, and the controls could be lifted forthwith. But if the controls do not wholly prevent the rise in wages, or are introduced too late to have that effect, there may be an *apparent* rise in real wages amid the shortage ; though consumption has to be diminished, the ratio of the money wage to the controlled price-level rises. In this case, a mere recovery of supplies to their old level will not immediately enable the controls to be lifted without a renewal of inflation. For though the old real wage could be re-established, the apparent real wage (in the sense of the ratio of wages to prices) cannot be made effective on a free market until productivity has risen further. I think one can detect an element of this story in our own recent experience. It is one of the drawbacks of using controls to meet a temporary emergency that a ' phoney ' relation of wages to prices can be established under the shelter of the controls, a relation which does not truly measure the real gains of labour ; yet it is this ' phoney ' relation which is sought to be maintained, in place of a maintenance of the real standard of living, when the controls are removed.

I turn to my second heading—relative wages in the narrower sense, the relation of one wage to another. If we have to take it for granted that it is hardly possible, under the Labour Standard, for the money wages of any group of workers to be *reduced*, it follows that troubles about wage differentials can themselves be a cause of inflation. In the simplest possible terms, if A-workers think that their wages should be 10 per cent. higher than those of B-workers, while B-workers think that their wages should be equal to those of A-workers, the satisfaction of the claims of one party must necessarily give rise to claims by the other. If both sets of workers are employed in the same industry, a series of concessions to such claims may push the costs of this industry right out of line with those of other industries ; then, even under a Labour Standard, the industry will find itself in difficulties. But if troubles about differentials become widespread, this brake on the wage-rise may disappear. The increase in effective demand, which has to be induced in order to prevent the first wage-rise from causing unemployment, facilitates wage-increases elsewhere. The industries which have been left behind in the advance then become sensitive in their turn, both because of the inter-industry differentials which have come into existence through the sectional advance, and because of the rising cost of living. It accordingly appears, without going further, that though wage-differentials can give rise to a certain amount of soreness without causing more than localised trouble, they must, on the whole, be moderately acceptable if they are not, under a Labour Standard, to be themselves a source of inflation.

But how can wage-differentials be acceptable ? To those who feel the ethical urge to equality as a guiding impulse, the clearest thing about wage-differentials is that they ought always, if possible, to be reduced. Such sentiments have lately been expressed with cogency by Mrs. Wootton, the title of whose book [1] I have deliberately echoed in the title of this paper. Nevertheless, one can appeal to Mrs. Wootton herself for a demonstration that these sentiments, though they are without doubt *one* of the ' social ' forces at work on the wage-structure, are in practice overlaid by other ' social ' forces which work in the opposite direction. In practice, differentials do tend to be acceptable if they have custom behind them—if they are in accordance with what the people who suffer from them, as well as those who gain from them, have come to expect.

[1] *The Social Foundations of Wage Policy.*

Thus, in conditions when the level of money wages is, broadly speaking, stationary, the purely egalitarian urge to raise the wages of the lower-paid workers relatively to others is not often in practice very strong. It is true that egalitarian sentiments have more power when wages are changing. Especially if the impetus to a wage-rise is derived from the cost-of-living motive, it is recognised that a fall in real wages is a more serious matter for the lower-wage than for the higher-wage groups ; the former have a stronger case for a cost-of-living bonus than the latter. In such circumstances wage differentials do tend to narrow ; sometimes actually in money terms, but more often by giving both groups the *same* rise in money terms, in spite of the difference in the wages from which they begin. This has, in fact, the effect of diminishing the real advantage of the better paid, in view of the higher price-level.

Thus, if one looks entirely at these 'social' forces, custom is the static and equality the dynamic element ; time, especially when aided by inflation, might be expected to iron all differentials flat. Certainly there is a tendency in that direction ; but how far it can go depends upon the economic obstacles which may lie in its path. There are economic factors to be taken into account, though the precise way in which they can operate under a Labour Standard needs some consideration.

Let us begin from a state of all-round full employment of labour, in which there is yet no 'over-employment' ; the demand for labour equals the supply in all trades. Now suppose that wage-rates, in some particular trade, are raised to such an extent as would cause a substantial reduction in employment in that trade, if there were no expansion of demand in general. If, however, demand is expanded, the appearance of unemployment may be prevented. But the expansion of demand will have to take a particular form (involving special protection for the high-wage industry) if its difficulties are thereby to be overcome. Otherwise, though the demand for its products will, indeed, be expanded (so that employment of the high-wage labour will fall less than it would have fallen if the expansion had not occurred), the demand for other products will be expanded at the same time. Among the labour producing these products we have assumed that there is no unemployment. A moderate increase in demand will accordingly produce a situation in which there is some fall in employment in the A-trade (as we may call it) combined with an excess of demand over supply (involving unfilled vacancies) in other (B) trades. There are now two cases, according as the workers who are unable to get employment in the A-trade can, or cannot, step into the vacancies which have appeared elsewhere. If they can, there will be no net unemployment as a result of the change in relative wages ; all that will have happened is a transfer of labour from A to B. But if there are difficulties in the way of the transfer, it may be necessary to create a severe shortage of labour in the B-trades before the labour displaced from the A-trade can be absorbed. The maintenance of full employment then implies the creation of severe labour shortages.

But let us take the milder case first. It is highly probable that the situation implied in this case is no novelty. If the workers in skilled trades, by the use of bargaining power, push their wages to an unnecessarily high level, they limit the scope for employment in those trades, but they do not necessarily cause any labour to be unemployed ; all that happens is that some people, who might have got employment in those trades, have to seek it elsewhere. This sort of thing could occur under the old conditions, and it can still occur under a Labour Standard. Differentials which owe their origin to no better cause than this can last a long time, and become hallowed by custom. And differentials which did once have an economic function, but are maintained by custom after they have lost it, fall from the economic point of view into a similar category.

It is obviously probable that the wage-structure, at any particular time, will contain many 'unnecessary' or 'uneconomic' differentials of this type ; but their identification can never be a simple matter. The test which the economist would be inclined to use, whether or not a reduction in wages would increase

employment and output in the trade affected, is not easy to use, nor is it in all cases quite conclusive. It is true, if this test is satisfied, that the additional employment is a clear gain to the workers who move (for they need not move unless they think it is to their advantage to do so) ; while the consequential increase in output is a clear gain to the consumers who take the additional units (for they need not take them unless they conceive themselves to gain by so doing). It is true that these shifts are clear gains, while the loss to the workers who were previously employed (and whose wages are reduced) is in some sense ' balanced ' by a reduction in cost which is a gain to other parties. But though these principles may be accepted, some weight can nevertheless be given to the counter-contention that the lowering of (relative) wages may lower standards of skill and craftsmanship, in such a way as to wipe out in the longer run much or all of the short-run gain. Those concerned have a strong interest to make as much as they can of this counter-argument ; we can recognise that, and still admit that there are cases in which it does have a certain validity.

It does, nevertheless, seem clear, in the light of this analysis, that actual differentials are always, to a considerable extent, historically determined ; their correspondence with those which would ensure maximum efficiency in the distribution of labour cannot be expected to be very close. It follows that the ' squeezing ' of differentials (which we have seen to be a feature of cost-of-living inflation) has some economic as well as social function. It squeezes out differentials which have become fossilised ; in doing so, it serves the cause of efficiency as well as that of distributive justice.

All the same, the fact that it does so is really no more than an accident. Since it is not itself impelled by any motive of economic efficiency, it can easily go too far. If it overshoots the mark, and reduces differentials below the amounts which are economically justified, we get a new problem, which is, in fact, a problem of the second case which I left over a little while back, and to which I must now turn.

If the differential between skilled and less skilled is fixed too low, so that it is the relatively low-wage labour which is overvalued, an intensity of demand which is sufficient to maintain full employment among skilled workers will be insufficient for the full employment of the less skilled ; an intensity which is sufficient to employ the less skilled will cause a shortage of labour in the skilled trades. An intensity which is between these two limits will leave some unemployment among the less skilled, and some unfilled vacancies among the skilled ; but the less skilled will *not* be able to move over to fill these vacancies. Those who are unemployed are unlikely to be suitable to fill the vacancies, even if they are trained ; while those who would be suitable will not move, because the incentive is insufficient. It is, accordingly, impossible, if differentials are too narrow, to maintain full employment without creating a scarcity of skilled labour.

Such a scarcity need not be inflationary, but it is only too likely to work out in such a way that it will be. For it is necessary, at this point, to distinguish between those trades in which the collectively agreed rates have a close correspondence with actual earnings, and those in which the agreed rates are minima, so that actual earnings can easily move out of line with what has been collectively fixed.[1] If the skilled trades, whose wages had become relatively depressed, were all of the first class, then (so far as I can see) the shortage of skilled labour would create inefficiencies, but would not lead to any general movement in wages. If, at the other extreme, they were all of the second class, it would be impossible to reduce *earnings* differentials below the amount which was economically justified. Any attempt to do so would merely cause the earnings of the higher-wage group to rise above their minima ; the market for their labour, under the conditions of scarcity, would become a competitive market. In fact, however, we have neither of these extreme cases ; the actual situation is a mixture of the two, some skilled trades belonging to

[1] The importance of this point was brought home to me by the studies of Mr. Kenneth Knowles, which have appeared in the *Bulletin* of the Oxford Institute of Statistics from 1951 onwards. (See in particular his ' Structure of Engineering Earnings,' *Bulletin*, September 1954). But this is not the only place in which my reflections have been stimulated by a consideration of the facts which Mr. Knowles has so ably brought together.

one type, some to the other. The effect of labour scarcity must then be a rise in earnings in the ' free wage ' trades ; but this creates a disparity (which is felt to be a disparity) between them and the other skilled trades in which wages are ' fixed.' Conditions thus become favourable for a rise in wages in the ' fixed wage ' skilled trades; and such a rise, in the circumstances supposed, is not without economic justification. But these ' fixed ' wages are a part of the regular structure of agreed wages ; a rise in any part of it tends to produce a general tendency to an upward movement, working through the ' social ' links which tie the various parts of the structure together. This is the way in which an attachment to ' wrong ' relative wages can be an inflationary element ; its inflationary influence cannot be removed (consistently with full employment) until people become prepared to accept economically reasonable differentials—differentials which are sufficient, that is, to ensure that lasting shortages of important classes of skilled labour are avoided.

I would emphasise the word ' lasting.' It does not follow from my analysis that a temporary scarcity of some particular kind of skilled labour, even if it leads to a sharp increase in the earnings of that trade, out of line with those of other comparable trades, need have very far-reaching effects on the wage-structure in general. Since it takes time, often a considerable time, to select and train additional workers for any specialised trade or occupation, it must be expected that the wage (or earnings) which will equate supply and demand in the short run, after a rise in demand has taken place, will be higher than that which would induce a sufficient supply in the long period. This is, indeed, one of the common ways in which unnecessarily high differentials come into existence ; wages are pushed up at a time of initial scarcity, and they fail to come down at the later stage when there has been time for the supply of labour to become more ample. Ideally, what should happen in such cases is that the agreed minimum should be kept in relation with the long-run supply-price of labour, while the incentive to move (only temporarily required) should be given by a temporary rise in wages above the minimum. For sufficiently temporary scarcities, one can perhaps detect a tendency for the wage-system to be operated in something like this manner. It is, indeed, more in accordance with trade union principles that negotiated wages should reflect what can be maintained as a regular thing over a period, than that they should fluctuate up (and, at least relatively, down) with every fluctuation of demand and supply on the labour market.

This brings me to my last heading, profits—on which I shall have to be brief, and must, therefore, do much less than justice to the subject. It is obvious that when businesses are earning high profits they can be more easily brought to concede claims for higher wages ; it follows for this reason alone that it is in conditions of high profits that claims for higher wages are most likely to be made. But under the Labour Standard we have the complication that profits in general are not compressed by a rise in wages in general ; thus if it should be the case that the level of profits necessary to induce full employment was itself so high as to call forth wage claims automatically, indefinite wage-inflation would be inevitable from this cause alone. But though it is easy to make our flesh creep with this argument, the assumptions on which it is based are not really very convincing. If wages were centrally negotiated, between a Grand National Trade Union and a National Employers' Association, inflation might indeed proceed in this manner for a time ; but the futility of the proceeding would rapidly become apparent, so that the wage-bargain would have to be merged in a treaty which would cover far more than the determination of wages. But as things are, both the structure of the trade union movement and the variety of the systems of wage-payment that are practised (on the whole for good reasons) in different trades, make such centralisation exceedingly unlikely. So long as wage-bargaining remains sectional, the profit comparison, though it must always be an element in the determination of wages, can hardly become so dominant as it has been assumed to be in the argument to which I have just been alluding.

Though wage-claims on other grounds are more likely to be pressed in conditions

of high profits, a direct claim for higher wages on the grounds of the profits that are being made by employers is a dangerous claim to make. It can so easily be turned the other way up. If it is laid down as a rule, that wages should go up when profits are high, the consequence, that they should not go up (or should even go down) when profits are low, cannot easily be resisted. This is a lesson which trade unions have had much opportunity to learn in the past ; and on the whole their history would suggest that it is a lesson which they have learned. Even in conditions of full employment, there remain enough industries with relatively low profits to ensure that the lesson will not quickly be forgotten. The principle that it is wiser to put forward wage-claims on other bases than the profits of the employer is one which may be reckoned, for British trade unions at least, as a permanent acquisition. It is an acquisition for which (I believe) the economist has every reason to be thankful. Not only does it diminish the danger of inflation ; it has also some effect in diminishing the danger of business monopolies defending themselves by a *de facto* collaboration with labour monopolies —handing over to their employees a share in their monopoly profits, in order to have the help of their employees in defending themselves against potential competition.

In its relation to the present situation of this country, the argument of this paper has been intended to be rather optimistic. For it is implied in what has been said that the continual rise in money wages since 1945 is not a necessary consequence of full employment (or of the Labour Standard) ; it is sufficiently explicable in terms of factors that are peculiar to the time through which we have been passing. Especially over the last four years, the main factors pushing up wages have been (1) the dismantling of the controls, with its somewhat ' phoney ' effect on the cost of living, and (2) the difficulty that has been experienced in the establishment of a new pattern of relative rates after the war-time disturbance. But these are difficulties which, in the absence of external shocks, we can expect to overcome. A condition in which the wage-level has again become relatively stable, so that movements in basic rates are exceptional rather than normal, should be coming into reach. Even if this expectation is too optimistic, I would still contend that it is for this condition that we must strive, and for which we should do our planning. For it can hardly be doubted that any serious disturbance to our rate of progress would itself push the level of money wages, under conditions of full employment, in an upward direction. If this push is superimposed upon a wage-level that is otherwise stable, then (within broad limits) it can be borne ; but if it is superimposed upon a wage-level that is already rising as fast as is tolerable, the additional strain of a more rapid rise must be very dangerous.

THE INFLUENCE OF THE ENGINEER ON SEA TRANSPORT AND TRADE

Address by

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THE seaports of the West Country will long be famed for the part their seafarers and small but sturdy ships played in voyages of discovery. Such enterprising expeditions opened up trade routes to new countries which have been developed to advantage by successive generations. By the end of the eighteenth century, however, the expansion of world commerce was only making slow progress. Hostilities between European countries reduced trade to a low level. In the absence of foreign competition British shipowners were complacent. Moreover, they were protected by Navigation Acts which compelled imports to be carried in British ships and by the monopoly of the rich Eastern trade by a chartered company. Healthy competition is, however, an essential factor in progress, and the stagnant conditions of those days removed incentive both in shipbuilding and ship operation.

Throughout the eighteenth century naval architecture remained static and improvements were negligible in both the speed and size of sailing ships. At the end of the period, the Americans having achieved political independence, were the first to break with tradition in the design and construction of merchant ships. They exploited the merits of fast, sleek sailing-ships in place of lumbering heavily armed merchantmen, and this move soon proved a technical and commercial success. Their sailing clippers were superior in speed, size and construction and were produced at about one-half the cost of European ships. The American shipping companies therefore captured the trade of the Atlantic and Pacific and a considerable proportion of the desirable produce of the East reached Europe via Boston.

In 1816 they were the first to establish regular sailings, and a monthly service between New York and Liverpool commenced. The average times of Atlantic crossings were reduced by some 25 per cent. and most companies averaged twenty-two days for the eastward run and thirty-five days for the return. These excellent services captured the major part of the immigration traffic to the North-American continent, while British shipowners looked on with indifference. This outlook might have proved fatal, for by the 1840's the American merchant fleet had increased until it was the second largest in the world and comprised sailing ships which were second to no others. Moreover, their fifty years' sole experience in the construction and development of speedy clippers gave them an almost unsurpassable lead in sail.

Pioneer engineers had other ideas of the best means for propelling ships, and as early as 1801 Symington patented his method of constructing a steam engine which provided rotatory motion at a crankshaft through a piston rod and connecting rod, thereby eliminating the conventional lever or beam. A horizontal single-cylinder double-acting engine of this type was used to drive the crankshaft of the stern paddle-wheel steamer *Charlotte Dundas*. The successful trials of this first practical steam-boat proved the potential merits of steam propulsion and inspired engineers on both sides of the Atlantic.

In 1807 Fulton employed British steam machinery to propel the paddle steamer *Clermont* on the Hudson River. This ship demonstrated the advantages of steam propulsion over sail for navigation on inland waterways, and early American

developments concentrated on this aspect. In 1811 Bell introduced the famous paddle steamer *Comet* into service between the ports of the Firth of Clyde. Shipbuilders quickly appreciated the qualities of this ship and comparable vessels were soon building at all major British ports. With limited sheltered waters these steamers were used largely for coastal service and by 1826 appeared on the London to Portugal and Dublin to Bordeaux routes. The working pressures of boilers were below 5 lb./in.^{2} and coal consumption of the steam machinery were little better than 10 lb./i.h.p. hour. In consequence the machinery and fuel occupied a large proportion of the available space in the ships and severely limited the freight-carrying capacity. Nevertheless, these steamers were a success and in the 1830's few steam-ship companies lacked financial support. Owners generally ploughed profits back into the companies and rendered a valuable service to commerce and steam-ship development by increasing their total tonnage with new construction. The burden of this practice was heavy but eventually proved sound policy, when, in 1837, the Government contracted private companies for the sea-carriage of mails.

Shipowners and engineers aimed at the conquest of the Atlantic by steam and so compete with the fast sailing-ships for the steadily growing passenger and freight traffic. By 1838 improvements in steam machinery had reduced coal consumptions to 5 lb./i.h.p. hr. and the total fuel necessary for the crossing could now be carried. In that year the *Sirius* steamed continuously from Cork to New York in nineteen days, but within a few hours of her arrival the Bristol-built *Great Western* arrived having steamed continuously from her home port on a maiden voyage of only fifteen days. The latter ship undoubtedly stole the thunder and Sir John Rennie considered, at that time, that her performance was such that there seemed no bounds to the extension of steam navigation. That the first voyage of the *Great Western* was in no way abnormal is evident from her eight years' service prior to being broken up ; a period in which she made seventy-four double crossings of the Atlantic with average outward and return times of sixteen and a half and thirteen and a quarter days respectively. Further, this ship operated at a substantial profit and left no doubts that the future of ocean-going ships lay in navigation by steam.

Constructional engineers and forgemasters had also been active since 1815 when a small sailing-ship with a wrought-iron hull plied the Mersey. This was followed three years later by an iron passenger-boat, *Vulcan*, which commenced operation on the Firth of Clyde canal, while an iron steamer, *Aaron Manby*, navigated from Thames to Seine in 1822. Such small iron vessels were initially prefabricated in sections at the forges of the Midlands for transportation and assembly at the ports. As confidence grew, larger ships were built in the shipyards, and by 1850 Lloyd's Register Book contained the names of no fewer than one hundred iron ships, more than half being steamers. Their size was generally below 550 tons, but the then colossal iron steamer *Great Britain*, of 3,618 tons, had already been in service for five years. Although a commercial failure, this venture was in many ways a technical success, establishing an inevitable trend to larger ships for ocean service. Moreover, her capacity to carry 1,000 tons of coal, 1,200 tons of cargo and 260 passengers represented a marked advance in the capabilities of the steamship.

The *Great Britain* was also the first large screw steamer to enter ocean service, and, although at an early stage of development and therefore less efficient than paddles, screw propulsion proved superior as heavy seas could damage paddles and render them inoperative. The Admiralty were prejudiced against both iron construction and screw propulsion and on their instructions the early Cunarders which carried the Atlantic Mails were wooden-paddle steamers. It was not until the 1860's, when a reduction in the hazards of fire, grounding and collision had been attributed to iron construction, that relaxations were permitted. At the same time experiments instituted by the Admiralty had improved the performance of propellers to a stage that these, too, could be accepted.

Each engineering achievement up to this time had been countered by the Americans, who maintained faith in the

sailing-ship. They produced bigger and faster clippers, and although fighting a losing battle on the Atlantic maintained superiority on the longer sea-routes. This was evident when passenger and freight traffic to Australia increased rapidly following the discovery of gold in 1854. British owners, freed from the restrictions of the Navigation Acts, purchased American clippers for their Australian services.

Although developed to a reasonable state of reliability, single expansion engines exhausting to surface condensers showed little improvement in fuel consumption. Steamers were, therefore, incapable, of making the long voyages to Australia via the Cape, and completion of the journey in stages was impracticable. Two alternatives were tried and both these were originated by the famous engineer Brunel and the equally eminent ship designer Scott Russell. The first was an iron ship, *Victoria*, of 3,000 tons, equipped with a small steam-engine driving a two-bladed propeller through gearing. The hull of the vessel was of greatly advanced design and a speed of ten knots could be obtained when the engines were operating, while six knots was maintained under sail. A voyage of sixty days from Gravesend to Adelaide won the Colonial Prize of £500 for the fastest journey. The second ship, the phenomenal *Great Eastern*, never confirmed the theoretical considerations on which her construction was founded. Brunel was of the opinion that this ship of 32,000 tons and carrying 10,000 tons of coal could steam the round trip to Australia without recoaling and, at the same time, carry sufficient cargo and passengers to provide a handsome return. The complete commercial failure of this ship is a well-known story, but she must be credited with laying more than 100,000 miles of cable, which established communications across the Atlantic and offered shipowners a convenient means of arranging cargoes and controlling the voyages of their steam ships.

It was abundantly clear that increased trade and traffic with the Far East and in the Pacific hinged on the improved economy of the main machinery of steamships. By 1854 advancement in the design and operation of iron boilers made working pressures of 25 lb./in.^{2} possible and also enabled Elder & Randolf to produce a practical compound engine in which the steam underwent a second stage of expansion. The first engine installed in the *Brandon* showed a fuel consumption of 3¼ lb./i.h.p. hr., but early engines of this type suffered from want of reliability. Two years later the Pacific Steam Navigation Company put two paddle steamers, *Inca* and *Valpariso*, into service and each was fitted with compound engines. Three other ships of this company were then re-engined with this type and one of these ships, the *Bogota*, displayed a 50 per cent. fuel saving after re-entry into service. Up to 1866 Elder & Randolf alone had built forty-eight sets of compound engines which were mainly operating in the Pacific. At the same time the P. & O. Company had ten ships propelled with compound engines in general operation and the Blue Funnel Line had three. The latter company were able to run 8,500 miles from England to Mauritius without recoaling.

A century ago Bessemer patented the converter process for the manufacture of steel and the products of early practice were used almost exclusively for hull construction, the quality not being found consistently reliable for boiler-making. Nevertheless, valuable experience was gained in the manipulation of steels in the shipyard and hastened the extensive use of the high quality and cheaper materials made available in quantity by the later Siemens Martin open-hearth process. Steels led to a minor revolution in shipbuilding and boiler and machinery construction. The superiority of steel over wrought iron permitted a 25 per cent. reduction in the scantlings of principal component parts representing a saving in weight of over 100 tons per 1,000 gross tons of ships, as well as reducing power plant weights. These savings were important at a time when steamers had to carry large quantities of fuel. An even greater effect of steel construction was evident in boiler practice, however, for within a few years working pressures increased threefold and brought about improved economy and reduction of unit size as well as enabling designers to develop more efficient and higher powered steam reciprocating engines.

Some indication of the progress which

occurred in a twelve-year period may be observed by the comparison of typical liners. The iron ship *Oceanic*, of 3,800 tons and completed in 1871, was one of the first ships on the Atlantic run to be equipped with compound engines. These engines of 3,000 i.h.p. received steam at 65 lb./in.^{2} and provided the ship with a speed of 14 knots. The *Umbria* and *Ethruria*, of 8,120 tons, were both steel ships completed by 1884. Engines of similar type developed no less than 14,500 i.h.p. for a single screw, steam being supplied at 110 lb./in.^{2} pressure. Whereas the *Oceanic* could cross the Atlantic in eight days, the latter were able to complete the same voyages in six days.

These ships represented the peak of the compound engine, in a relatively short but most important period of predominance. Increased boiler pressures enabled expansion of the steam to be carried a stage further and triple expansion engines made an appearance. This engine appeared in 1872 but the true merits were first demonstrated in 1881 when the *Aberdeen*, of 4,000 tons, completed a spectacular voyage. This ship, which was also fitted with sail, had a three-crank triple expansion engine designed by Kirk and with steam at 125 lb./in.^{2} produced an i.h.p. of 1,800. Carrying 4,000 tons of coal and cargo this vessel steamed to Melbourne in forty-two days, replenishing the bunkers at the Cape. On long voyages the coal consumption averaged 1·7 lb./i.h.p. hr. compared with 2¾ lb. for compound engines. The triple expansion engine installation possessed the characteristics which provided the solution to sea-trade with the Far East and expanded trade with these regions as the compound engine had done in the Pacific and the simple steam-engines in the Atlantic. In addition, this type of engine soon predominated other steam reciprocating types and has retained that position up to the present time.

During the 1880's triple expansion superseded those of compound type on the Atlantic. At the same time multi-screw propulsion was adopted to meet the demands of steam-ship companies for greatly increased power of single ships. Moreover, competition was keen for the trade and passenger traffic, and speed and size of ships were matters of international rivalry. It was these events which led to the realisation of Brunel's conception when he planned the *Great Eastern*, but forty years of engineering progress had to pass before similar ventures could be undertaken without the risk of complete commercial failure.

Of the liners fitted with triple expansion engines the twin-screw *Campania* of 12,950 tons and 601 ft. in length, may be quoted. Built in 1893 the engines developed a total of 31,000 i.h.p. and propelled the ship at 22 knots. About the same time the quadruple expansion engine was evolved and offered further economy in operation. The largest steam reciprocating engines built were of this type and engined the liner *Kaiser Wilhelm II.* 43,000 i.h.p. was obtained with steam raised at 225 lb./in.^{2} pressure in fourteen double-ended and seven single-ended cylindrical boilers. Power weight ratios of large reciprocating machinery installations ranged from 6 to 7 i.h.p. per ton and the total weight of the quadruple machinery of the *Deutschland* weighed no less than 5,670 tons for 38,900 i.h.p. In addition, unpleasant hull vibrations were excited by the heavy reciprocating masses and were detrimental to the comfort of the passengers. For these reasons steam reciprocating machinery was not entirely suitable for high-powered liners and the limit of total power of a ship had been attained so far as these prime movers were concerned.

It was not surprising, therefore, that Parson's *Turbinia* was acclaimed as an outstanding triumph, for it offered the solution to these problems. Nine years later, in 1905, the 30,000-ton liner *Carmania* was engined with steam-turbines of 21,000 s.h.p. and after a further two years the quadruple-screw liner *Mauretania* was completed with single reduction geared turbines of 70,000 total s.h.p. A speed of 24½ knots won this liner the Atlantic record which she retained for a twenty-two-year period signifying that she represented the end of the first era of national and international rivalry for the largest and fastest ship afloat.

During this era shipowners had also been able to equip their fleets with ships of increasing size, for expanding trade had

always proved their speculation sound. The British Merchant Fleet had increased by no fewer than 5,000 ships in half a century and in the latter half of this period other nations had recognised the achievements of British engineering and built up fleets of merchant steam-ships for their own purposes. In consequence, there was now a tendency to adopt economical sizes of each type of merchant-ship and many wondered whether a ship like the *Mauretania* could be a paying proposition. Further, transocean cables which had been laid by steam-ships and the recently introduced wireless telegraphy enabled ships to operate more economically by reducing the delays at ports. Owners were, therefore, able to prearrange cargoes and ascertain the exact whereabouts or progress of the ships in their fleet. On the other hand, the rapid growth in world merchant-ship tonnage and the availability of telegraphic communication stimulated competition and full cargoes were not readily obtainable.

For the engineer the conquest of the trade routes by steam navigation was complete and the steam reciprocating and turbine machinery had attained a high degree of reliability with reasonable economy. His role had therefore changed from a pioneer to a consolidator ; adapting, modifying and developing the various types of machinery at his disposal to meet the many requirements of shipowners and operators.

By the end of the nineteenth century the supplies of heavy oil fuels were sufficient to warrant consideration as an alternative to coal for firing the boilers of steam-ships. The advantages of liquid fuels in respects of transportation, handling and storage were obvious, but it was not until 1894 that an oil-burning steamer, the *Baku Standard*, entered sea service. In the first half of this century coal burning has been almost entirely replaced by oil burning for sea-going steamers and so far as new construction is concerned, the change-over is virtually complete.

The next landmark in marine engineering was the oil engine which was introduced as a main prime mover for ocean-going merchant ships in 1910, when the 1,179-ton tanker *Vulcanus* was completed and engined with a Werkespoor 6-cylinder 4 S.C.S.A. diesel engine of 500 b.h.p. rating at 200 r.p.m. It may be of interest that this engine was still operating satisfactorily when the ship was sold for scrap in Japan, twenty years later. A greater advance was represented by the Burmeister & Wain diesels which engined the twin-screw motorship *Selandia* of 5,000 tons. Completed in 1912 each 8-cylinder 4 S.C.S.A. engine developed 1,050 b.h.p. at 140 r.p.m. and showed that the problems of high-powered oil engines were being effectively overcome. This installation was so revolutionary as to attract the attention of the world. Sir Winston Churchill, then First Lord of the Admiralty, visited the ship and examined the machinery layout. On this occasion he said that . . .' it was a pleasant duty for him to congratulate Denmark, the ancient seafaring nation which had shown us the way and taken the lead in a venture that will form an epoch in the development of shipping.' As on many other occasions, history has confirmed the validity of his shrewd judgment. The *Selandia* was in operation until 1942 when she was wrecked. During the life of this ship the oil engine had become the predominant prime mover for new merchant-ships constructed in the world, and by 1939 engined more than one quarter of the total number of ships afloat.

Following the 1914–18 war, shipowners were faced with the problem of rebuilding their total tonnages to make good the losses during the hostilities. They had struggled to achieve this aim without subsidy when trade eased and competition became keen to secure freights. Reductions in fuel costs were of vital importance and it was this aspect which received the attention of engineers. The triple expansion steam-engine then operating with saturated steam supplied by Scotch boilers of 200 lb./in.^{2} working pressure, offered fuel consumptions of 1·6 lb. of coal or 1·1 lb. boiler oil/i.h.p. hr. Steam-engine makers soon developed improved designs incorporating such features as poppet valve gear and uniflow exhaust of cylinders, and capable of operating with superheated steam at total temperatures approaching 700° F.

The inability of the reciprocating engine to utilise all the possible expansion of the

steam due to limits imposed on the size of the L.P. cylinders, was overcome by the fitting of exhaust steam-turbines. This was no new feature, for Parsons used a reaction turbine supplied with exhaust steam from two reciprocating engines and driving an independent shaft directly, in the triple-screw steamship *Otaki* completed in 1908. Before this arrangement was superseded by the geared-turbine for passenger ships, a number of other installations were completed, including those of the *Olympic* and *Titanic*.

The arrangement first fitted in a single-screw ship in 1926 was the Bauer-Wach system, in which the drive from the turbine was transmitted through double-reduction gearing and a fluid-coupling back to the reciprocating engine main shaft. Other manufacturers preferred electrical to mechanical transmission and in these cases the exhaust steam turbo-generator with either a.c. or d.c. output supplied an electric motor mounted at a convenient position on the main propulsion shafting.

Reheating of the steam before admission to the M.P. cylinder of the reciprocating engine also offered appreciable improvement in performance. The direct method of raising the conditions of the H.P. exhaust employed a tubular heat exchanger supplied with superheated steam direct from the main boiler unit. As an alternative, an exhaust turbine was used to drive either a d.c. generator which supplied electrical heating elements or a steam compressor.

Each of the improved steam reciprocating installations had its own particular merits and champions. Fuel consumptions of just under 1 lb. coal or 0·8 lb. oil/i.h.p. hr. could now be offered and these represented appreciable savings in overall fuel costs. Nevertheless, progress in the oil-engine field had been so marked as to offer a serious rival to the steam reciprocator.

In 1914 Messrs. Doxford & Sons, Ltd., of Sunderland, ran endurance trials on a single cylinder, 2-stroke cycle, heavy oil engine which embodied the opposed piston principle and developed 450 b.h.p. at 125 r.p.m. This represented a major advance in the power output of a single cylinder. During the 1914–18 war the performance of both 4-stroke and 2-stroke oil engines progressed rapidly and this was assisted by the adoption of solid injection of the fuel; an important departure from the Diesel principle.

By 1921, 2-stroke types of 4-cylinders were providing 3,000 b.h.p. on a single screw but 4-stroke types nevertheless predominated in marine practice. Four years later, double-acting engines of the latter type displayed single cylinder outputs of 1,100 b.h.p. The twin-screw motorship *Gripsholm*, for example, was directly propelled by two 6-cylinder engines of this type, 33 in. in bore and 59·1 in. stroke, with a total output of 13,500 b.h.p. at 125 r.p.m. Doxfords soon challenged this achievement by building engines of the 2-stroke type described and the 5,000 total b.h.p. represented 1,250 b.h.p. per cylinder.

At this time many of the service problems experienced with 2-stroke cycle oil engines had been surmounted and a number of engine builders who had previously favoured 4-stroke types now regarded the former as a better proposition. In consequence the passenger liners *Britannic* and *Georgic*, completed in 1930 and each fitted with two 10-cylinder engines of 20,000 combined b.h.p., represented a peak in the progress of the 4-stroke type. Thereafter preference for the various 2-stroke engines prevailed until the present day, when they form no less than 80 per cent. of the main oil-engines fitted in ships. Further, the fitting of Buchi Superchargers to engines of the 4-stroke type, although giving improved performance, had little effect on the predominance of the 2-stroke engines.

By 1934 2-stroke engines were supplying up to 7,000 b.h.p. on a single screw and the replacement of cast-iron bed plates, entablatures and columns by welded fabrications in mild steel did much to reduce the weight and size of the engines. In addition, renewed attention was given to the use of exhaust gases to raise steam in waste heat boilers which augmented the supply from the donkey boilers and reduced the overall fuel consumption of the auxiliary machinery.

After that time the power of heavy oil-engines continued to increase steadily. In post-war years, however, the successful adaptation of a moderate degree of supercharging in 2-stroke engines had provided

30 to 35 per cent. increase in power with m.i.p.'s of up to 130 lb./in.^{2}. Engines are already under construction with single cylinder outputs of 1,650 b.h.p. and further increases to 2,200 b.h.p. are contemplated. Large slow-running engines therefore develop 10,000 to 17,000 b.h.p. and offer a serious challenge to the geared steam turbine for the propulsion of single-screw tankers and cargo ships. Without increase in the weight of the installation, two medium-speed oil-engines coupled through 2 : 1 reduction gearing to a single shaft have also been employed to develop these powers. The gearing is protected from engine excited torque fluctuations by either fluid or, more recently, electro-magnetic couplings installed between engines and pinions. Moreover, such arrangements show considerable advantages during manoeuvring. Although 2-stroke engines are usually used for a propulsion unit of this type, highly supercharged 4-stroke engines are also available when slightly lower powers are required.

The following ships have machinery installations which illustrate these points :—

(1) The single-screw motorship *Polarbris* is directly propelled by a 6-cylinder Doxford engine which develops 8,000 b.h.p. at 104 r.p.m.

(2) The M.V. *Caraibe* employs two 2-stroke engines developing 14,000 b.h.p. at 215 r.p.m. to drive a single shaft running at 117·5 r.p.m., through electro-magnetic couplings and single reduction gearing.

(3) The cargo liner *Lichtenfels* is fitted with two highly supercharged 4-stroke oil engines of 5,600 b.h.p. at 250 r.p.m. which is transmitted through fluid couplings and reduction gearing to a single screw running at 105 r.p.m.

These single-acting engines of cross-head type built by M.A.N. represent the latest trend in this class of 4-stroke design. A b.m.e.p. of 227 lb./in.^{2} is obtained by supercharging at 35 lb./in.^{2} pressure from a 14-stage axial compressor with intercoolers and driven by a 3-stage exhaust gas turbine.

High speed, medium power 2- and 4-stroke supercharged engines of vee and delta type with up to 24 cylinders are also available for merchant-ship propulsion. Multiple units geared or electrically connected to a single screw, offer advantages of reduced space requirements and low total weight per h.p. developed. Shipowners prefer the slower-running engines, however, and they are not yet convinced that engine changes and shore repair and servicing establishments are the answer to rising maintenance costs.

Modern oil engines have excellent fuel consumptions of about 0·32 lb. oil/b.h.p. hr. which correspond with overall efficiencies of 40 per cent. The most important feature of slow and medium speed engines has been their recent conversion for burning boiler or residual grades of oil, however. The use of these fuels can provide a saving in fuel costs of up to 30 per cent. This achievement has given the oil engine a substantial lead over rival prime movers but the approach towards the theoretical efficiency leaves little scope for greater economy.

Before the 1914–18 war, water-tube boilers had superseded those of cylindrical types in warships and had proved superior in performance and operation. The working pressure of boilers in merchant-ships advanced slowly during the first quarter of this century and the maximum employed was little in excess of 250 lb./in.^{2} Over this period the cylindrical boiler predominated throughout the whole power range of steam machinery. A considerable number of water-tube boilers were installed in merchant-ships during the Great War, however, but their satisfactory service did little to dispel the prejudice against them except in America, where war-time construction comprised the major part of the merchant fleet. The development of economical steam-turbine machinery installations during the 1920's demanded H.P. inlet conditions which could no longer be met by cylindrical boilers and the water-tube type now controlled the field of high-pressure and high-temperature steam production.

The benefits of advanced steam plants were soon evident by the appreciable reductions in specific fuel consumptions. The S.S. *Duchess of Bedford*, completed in 1928, was fitted with Yarrow boilers with a working pressure of 340 lb./in.^{2} at

670° F. total temperature. The fuel consumption of the main machinery was recorded as 0·57 lb./s.h.p. hr. When compared with a Scotch boiler installation of eight years earlier a fuel saving of at least 15 per cent. was evident. Three years later the *Empress of Britain* entered service and her full power of 60,000 s.h.p. was developed by turbines supplied with steam at 425 lb./in.² and 725° F. On trials the same fuel consumption was recorded, but in this case it represented the all-purpose requirements. An appreciable advance in the use of higher steam conditions occurred in 1936 when construction commenced on the turbo-electric tanker *J. W. Van Dyke*. The two Babcock & Wilcox boilers of this ship were designed for a maximum working pressure of 675 lb./in.², the superheater outlet steam being at 625 lb./in.² and 835° F. The fuel consumption of the main machinery was approximately 0·51 lb./s.h.p.hr. In the sister tanker *Robert H. Colley*, laid down in 1938, the steam temperature was raised to 910° F., which was the most advanced condition used in merchant ships in the pre and early war years.

Some remarkable high-pressure installations were also completed, notably the 3,200 lb./in.² of the Benson boilers in the *Uckermark*, 1,990 lb./in.² by Loeffler type in the *Conti Rosso* and the *Potsdam* with 2,650 lb./in.² working pressure. Such high pressures were rarer, however, and the main object has since been for pressures below 1,000 lb./in.² with high degrees of superheat.

Water-tube boiler makers were active in their design advancement and by 1938 boiler efficiencies of 87 per cent. were possible. This was partly responsible for the low overall fuel consumptions of propulsion machinery. Of equal importance were the increases made in total heating surface and the rates of evaporation produced by single boilers for this permitted boiler plant of reduced size and weight. In addition, high quality fusion welded joints replaced those of riveted type in the fabrication of boiler drums and by this means weight saving alone exceeded 1¼ tons per drum. As an indication of the merits of water-tube boilers the French liner *Normandie* may be cited. It was desired originally that cylindrical boilers should supply the steam for her 160,000 s.h.p. turbine machinery, but the size of the ship would have been such that no existing harbour could accommodate her. Twenty-nine water-tube boilers were used finally and steam was raised at 400 lb./in.² pressure and 620° F.

In America the Scotch boiler had practically died out by 1939, for a year later special arrangements had to be made to supply boilers of this type for sixty cargo ships ordered by Britain. In Britain, however, the cylindrical boiler has retained some of its popularity for both main and auxiliary steam generation, due to its rugged characteristics and it is usual practice to use water-tube boilers where steam pressures exceed 300 lb./in.² Steam-turbine installations at present engine ships of 50,000 s.h.p. per screw or 200,000 total s.h.p. and as greater powers will not be required for some years, it is natural that research should be concerned with reducing fuel consumptions well below 0·5 lb./s.h.p. hr. and developing more compact plants. Economical units incorporating one or more reheat points are at present under consideration and it seems that gas-fired reheaters will supersede those of steam type. Increased steam conditions at the H.P. turbine inlet still present a major source for improved economy. The tanker *Atlantic Seaman*, which has been in service for five years, has a turbine plant working with inlet steam at 600 lb./in.² pressure and 1,020° F. This machinery remains in advance of other merchant-ship installations and fabrication, design and service problems of high temperature materials are largely responsible for this position.

Of the ships at present under construction all in excess of 20,000 tons will employ geared steam turbines for main propulsion. Within the size range 100 to 20,000 tons, 70 per cent. will be propelled by oil engines, while the remainder, with isolated exceptions, will have steam reciprocating or turbine main machinery. Turbo or diesel-electric transmission have not proved popular although restricted gear-cutting machine capacity increased the number of installations of these types during the last war. In some instances, however, turbo-electric main machinery have been preferred for large oil tankers

in order that the main generator could be used to supply the cargo pumps when in port.

Both tankers and ore carriers have increased in size to meet the needs of supplying the expanding oil, chemical and metallurgical industries. Vessels of these types at present rival large passenger ships in size, but retain single-screw propulsion. To meet these conditions steam turbine manufacturers in particular have developed compact power plants developing up to 20,000 s.h.p. Further progress is continuing in this direction and double-reduction gearing in particular is the subject of extensive researches. The problems of successful transmission of gear tooth loads of two to three times recognised pre-war levels have not yet been satisfactorily solved and surface hardened and ground gears are at present under consideration.

In recent years other forms of prime mover have been considered for merchant ship propulsion, but so far these have not proved sufficiently attractive for general service.

The marine gas turbines at present under development for marine propulsion purposes are of open cycle type. In general, the H.P. turbine drives an axial compressor as an independent unit while the L.P. provides the useful output and exhausts through a heat exchanger which preheats the inlet charge to the combustion chamber. The designs vary considerably, but can be considered in two main categories. The first covers those of light-weight construction, under 7 lb./h.p. and developing up to 6,000 s.h.p. The H.P. inlet temperatures of about 820° C. limit the life of the hot parts to little more than 1,000 hours. Units of this type power light naval craft only, for a satisfactory life in excess of 20,000 hours is required for the main engines of merchant-ships. This is at present obtained by limiting the top temperature to below 700° C. in medium-weight plants of 25 to 30 lb./s.h.p. Both classes of gas turbine have overall efficiencies of about 20 per cent., but when the long service reliability of the latter has been established, advances in top temperatures and design should provide fuel consumptions below 0·5 lb./s.h.p. hr. Since these engines cannot be run astern, either electric transmission, reversible pitch propellers or reduction gearing with reverse trains must be used. Of these, the last quoted is receiving the greatest attention and is likely to predominate in future installations.

In France remarkable progress has been made recently in a power plant comprising a free piston gas generator and geared gas turbine. Turbine inlet conditions of four atmospheres pressure and less than 500° C. are used as the peak temperature of the thermodynamic cycle occurs in the cylinder of the free piston gas generator. Overall thermal efficiencies therefore approach those of existing oil engines and are represented by a fuel consumption of 0·39 lb. diesel oil/s.h.p. hr. The problem of manoeuvring does not apply in this instance as astern wheels are incorporated in the turbine shaft.

The size of the gas generator has been standardised so that equal reliability may be obtained with all units. High-power plants will, therefore, comprise multiple units supplying one or more geared turbines. The first main marine installation of this type was installed in the 800 tons d.w. *Cantenac*, completed in 1954, and other ships at present under construction will be similarly fitted.

The construction of two submarines for the U.S. Navy in which the heat developed in a nuclear reactor is used to raise steam for supplying both main and auxiliary turbine machinery, has resulted in wide speculation of the possible adoption of similar power plants for merchant-ship applications. Indeed such an impressive statement that the fissioning of 1 lb. of uranium produces heat equivalent to 2,300 tons of coal or 300,000 gallons of oil, raises visions of revolutionary changes in the design of merchant-ships and their machinery. As these changes would solve many existing problems, it seems that thermal reactors must eventually provide the energy for steam-ship propulsion. When this event will be possible depends on the rate of progress made in reducing to more attractive levels the weight, size and cost of reactors and screening and the satisfactory solution of control and maintenance problems. It appears that the power weight ratios of these units will be better suited to the higher-powered plants

of large cargo and passenger liners than to the many low-powered installations in smaller ships. Undoubtedly, the latter will continued to be engined with conventional prime movers.

In the submarine U.S.S. *Nautilas* purified water under high pressure forms the primary coolant which extracts the heat from the thermal reactor. The steam then raised in heat exchangers is at relatively low pressure and temperature, probably not more than 180 lb./in.^{2} and 440° F.; consequently, geared steam-turbines of moderate thermal efficiency are used and high temperature is confined to the primary coolant section. In effect, the controlling factor is the rate at which heat can be extracted from the small volume of the thermal reactor and providing this is adequate to meet the demands, thermal efficiency is of only secondary importance. Moreover, raising the thermal efficiency by adopting different type reactors and primary coolants which produce steam at higher temperatures and pressures does not result in longer duration for the same amount of nuclear fuel as with oil-fired installations. It is related to the size of plant, however, and for this reason fast reactors and liquid metal primary coolants may be superior for steamships.

While the developments of main machinery installations in little over 150 years have made a substantial contribution to the expansion of world trade, auxiliary machinery has also played a part. The luxury of a modern liner stems, in part, from this source. It is just over seventy-five years ago that Inman first employed electricity on board ship to supply arc lamps fitted in the saloon of the *City of Berlin*. From that time the applications of electricity increased markedly and the size of the d.c. generating plant grew to meet these needs. In more recent years a.c. systems have become prevalent in both oil tankers and passenger liners. In addition, diesel-driven generators have largely superseded those of steam reciprocating engine-driven types. Large liners invariably employ steam turbines for both main and auxiliary purposes, but the installation of the Greek liner *Olympia*, completed in 1953, departed from this practice by employing steam turbines for main propulsion only, the auxiliaries being exclusively diesel-electric. In cargo ships a new departure had been the use of a free piston gas generator supplying the gas turbine of a turbo-electric set and a number of normal gas-turbine sets are now under construction and will soon be in service at sea.

Many trades flourished with the introduction of refrigeration machinery for shipboard purposes. The meat trade originated in 1880 when the S.S. *Strathleven* arrived in London from Australia carrying 40 tons of frozen meat. This event was given prominence in the world press and laid the foundation of a new trade which was to grow at a remarkable rate. Prosperity was brought to Australia, New Zealand and South America, while the peoples of Europe were supplied with cheap foods, at least in pre-war years! New Zealand alone exported 760 tons of meat in 1882, three years later the figure was no less than 15,000 tons and doubled again in another decade. It can truly be said that few engineering achievements have had such revolutionary effects on world trade and prosperity. Today Lloyd's Register Book contains the details of nearly 1,000 shipboard refrigeration installations on which the fish, meat, fruit and farm produce trades depend.

Until recently the holds of the ship were the refrigeration chambers, but a few companies are introducing separate refrigeration containers. In effect, these comprise large portable refrigerators requiring only an electrical supply. Containers of this type offer considerable scope for the handling of delicate low bulk cargoes, but they are unlikely to replace the existing system for storing bulk frozen cargoes.

Possibly the most extensive use of auxiliary machinery arises in factory ships. The commercial advantages of employing a whale factory ship which can deal with the catches of a number of whalers has given impetus to the introduction of long-range fish factory ships. The *Fairtry*, completed in 1954, is the largest trawler afloat and incorporates modern factory equipment for dealing with the catch. This trawler has a stern opening and ramp and when the fish is landed automatic electrically operated

machinery undertakes the process of heading, filleting, skinning, cleaning and packing. Mechanical conveyors operate between the last operation and the deep-freeze chambers used for storage. With additional plant for cod liver oil and fish meal production, this trawler may well represent the future means of meeting the expanding fish industry.

This industry has many existing difficulties so far as the economic operation of distance water trawlers is concerned. Already existing trawlers have been modified to include deep freeze holds in which the catch can be stored. It is anticipated that the new system of storing the catch will overcome some of the problems of marketing in the peak spring and summer periods by enabling the surplus, stored in aluminium alloy containers, to be transferred and kept in shore refrigeration warehouses. Normally, the surplus must be disposed of to fish-meal factories, thereby incurring an appreciable loss.

An examination of the history of one particular field of engineering has indicated that the remarkable increases which have occurred in the prosperity of nations, during the last century and a half, can largely be attributed to the achievements of the engineer. It is also true that the countries which have encouraged engineers and industry enjoy the highest standard of living. Today we aim to improve our living standard still further and, at the same time, assist other countries to do likewise. That the engineer is continuing to play his part is evidenced by the high rate of technical progress maintained during post war years.

ANTHROPOLOGY—A SCIENTIFIC UNITY?

Address by

DR. D. B. HARDEN

PRESIDENT OF SECTION H

THE honour of being invited to become President of Section H must always bring pleasure to the recipient, but it must also bring to him a sense of the high standing of the office, and, more particularly of the great responsibility of choosing a subject for his address which may not only be of some interest to those who do him the honour of listening to it, but may also be of some value in forwarding the interests of the Section, and of the Association as a whole.

My own experience in casting about for a subject which would fulfil these two requirements led me to decide that I could best fulfil the second by a general discussion of the Section's own aims and activities, past and future. It is not, I hope, too much to assume that such a topic is of potential interest to my audience, and will, therefore, fulfil the first also, though how far I can arouse that latent interest must, of course, depend upon my own powers—such as they are—of thought and exposition.

Much of what I shall have to say may appear to you trite, but if so I hope you will still bear with me, for there are many truths and facts which cannot be repeated too often when an appropriate occasion arises ; and though most of you, if not all, may be well aware of the full import and scope of our Sectional subject and do not need reminding of its value as a scientific study and discipline, yet there are others, if not within this audience, then perhaps outside it, whom it is still necessary to convince that the many component branches of anthropology are all essentially scientific and need to be approached as such if they are to be studied aright, and to the best purpose.

That the officers and members of this Association 'for the Advancement of Science' have believed this almost from the start is made plain by a glance through the early programmes of meetings. The first appearance of an anthropological subject in the programmes is, so far as I can trace, in 1834, at the Association's 3rd annual meeting in Edinburgh, when J. B. Pentland read a paper on 'The Ancient Inhabitants of the Andes' in the Zoological Section. This was, it would seem, a bit premature, for it was not until five years later that our subject again figured in the programme. It appeared then, not only in the Geological Section, where W. R. Wilde read a paper on. 'The topography of Ancient Tyre,' but also in the Zoological and Botanical Section, where J. C. Prichard discoursed on 'The Extinction of Native Races.' Prichard in his paper expressed regret that so little attention was given to the ethnography or the natural history of the human race, while the opportunities for observation were every day passing away. The problem he had in mind was also exercising others outside the British Association's orbit, as is shown by the foundation two years earlier by Thomas Hodgkin and others of a society called 'The Aborigines Protection Society,' and Prichard concluded his paper by an appeal for support for that body. Prichard's paper was indeed a milestone on a specific anthropological road of which the end has not yet been reached, for his arguments led to a grant being made to a committee that was to draw up a questionnaire to serve as a guide for all who came into contact with native races. The Committee, comprising Hodgkin, Robert Owen, and Prichard himself, completed their work in 1843 by producing a questionnaire which was, in effect, a preliminary edition of the well-known *Notes and Queries on Anthropology*, of

which subsequent British Association committees produced further editions in 1852, 1874 (the first under its present title), 1892, 1899, 1912, 1929 and 1951. This latest edition, under Mrs. Seligman's editorship, was produced by the British Association and the Royal Anthropological Institute working together. The Association has now transferred to the Royal Anthropological Institute full responsibility for future issues.[1] That such a valuable and long-lived anthropological project should have been initiated in the name of Science by the Association as long ago as 1839 is firm evidence, if such were needed, that the scientific importance of our subject was fully realised from the start. No doubt it was largely due to Prichard's own influence and able advocacy that this happy result was achieved.

In 1843 the Ethnological Society of London was founded, and this clearly stimulated the provision of papers on anthropological subjects within the British Association. Before 1844 they are rare. In 1844 there were no fewer than 12 papers read in the Zoological and Botanical Section on subjects such as 'The Eastern Limits of the Australian Race and Language,' 'The Ethnography of Africa as Determined by its Languages' and 'The Sandwich Islanders.' That the first two of these titles mention 'language' is indicative of a trend of the times, and we find for many years to come that anthropological speakers in the British Association lay much stress on linguistics, both primitive and advanced.

Anthropology, or as it was called by the British Association at that time 'ethnology,' had now clearly come to stay and this was recognised in 1846 by the formation of a Sub-section of Ethnology within Section C (Geology and Geography), a Section which also included at this period papers on human palaeontology in its main Geological programme. This Subsection of Ethnology lasted till 1850 and during these five years the emphasis remained, as it had been in 1844, on ethnology proper and language. There were papers on 'The Aborigines of Newfoundland,' on 'The Races of the Cimbric Chersonese,' on 'The Malay Languages' and even on 'Oriental Words adopted in English.' There was as yet very little archaeology and what there was had an ethnological and physiological slant.

The next stage came in 1851 when Geography and Geology were separated and Ethnology joined Geography in forming a new Section, E. This union lasted till 1869. The emphasis remained, in the first place, on pure ethnology, whether of primitive peoples, e.g. papers on the racial affinities, manners and customs of various aboriginal tribes, or of more advanced peoples, with subjects such as 'The Ethnology of the Liverpool District,' or 'The Ethnic Relations of the Egyptian Race,' or even, in 1854, 'The Lost Tribes of Israel,' a paper of which the official Transactions print no summary! The second emphasis was on language, as before, with papers, for example, on 'Personal Pronouns in Indian, European, Syro-Arabic and Egyptian Languages,' on 'Negro-European Dialects of Surinam and Curaçao' and on 'Surnames of the Irish People,' though papers on language seem to get fewer as the years pass. The third main emphasis is now, at length, on archaeology—mainly British, and often dealing with local topics relating to the district in which the meeting was taking place. There is also some physical anthropology of primitive peoples and even a paper by J. Brent in 1851 on 'The Comparison of Athletic Men in Great Britain with Greek Statues.'

Meanwhile the new subject of prehistoric flints, which was initiated in Britain on a proper scientific basis in 1859 by Evans and Prestwich with their championship of the genuineness of Boucher de Perthe's Somme discoveries, found its way into Section C (Geology), though not, so far as I can trace, until 1862. A paper on the Neanderthal skull also appears in Section C rather than in Section E, in 1863.

As already mentioned, the Ethnological Society of London had existed since 1843. In 1863 the Anthropological Society of London was founded. Two years later the International Congress of Anthropology and Prehistoric Archaeology was initiated at Spezia and in 1868 it met in Norwich contemporaneously with the Association's own meeting there. It is clear, therefore, that in the 1860's there was a great surge of anthropological endeavour, but that anthropology and ethnology were

[1] Report of Council for 1948–9.

considered somewhat different, the one from the other. So far was this so that in Paris in 1859 two quite different bodies, the Societé d'Anthropologie and the Societé d'Ethnologie, were founded contemporaneously. In France, and, perhaps elsewhere, the differentiation lay along the lines of physical (anthropological) and cultural (ethnological) studies, but the most up-to-date thought evidently construed anthropology as the broader term, within which ethnology should rightly find a place.

That this was the feeling in the Association itself seems clear from developments in the late 1860's. First, in 1866, Section D (Biology) suddenly begets an Anthropological Department, that is, a Subsection. This, presided over by Alfred Russel Wallace, a friend of Darwin's and an authority on the Malay Archipelago, was clearly a success, although Section C (Geology) continued to carry papers on prehistory, and Ethnology remained with Geography in Section E. Despite its success the new Anthropological Department was discontinued in 1867 and 1868, perhaps because discussions were in progress which culminated in 1869, when Ethnology was divorced from Section E (Geography) and the 1866 experiment of forming an Anthropological Department within Section D (Biology), was confirmed. The first title of the new Department was ' Ethnology, etc.' but two years later it became, and thereafter remained, ' Anthropology,' the change occurring, curiously enough (but perhaps with firm intent), in the very year that the Ethnological and Anthropological Societies of London were united as the Anthropological Institute of Great Britain and Ireland.

At last all anthropology within the British Association was under one roof. No longer did Section E accept ethnological papers, nor Section C accept human palaeontology.

I do not know who were the prime movers in bringing about these changes but I would expect that A. R. Wallace, the President of Section D's Anthropological Department in 1866 and E. B. Tylor, the first President of the re-formed Department in 1869, both had much to do with them. Tylor's address in 1869 is not recorded, but Wallace's in 1866 is and part of the summary is worth quoting as it gives a sketch of the scope of anthropology, covering so much of what I want to emphasise to-day :[1]

' Anthropology,' he says, ' is the science which contemplates man under all his varied aspects, as an animal and as a moral and intellectual being, in his relations to lower organisms, to his fellow men and to the universe. The anthropologist seeks to collect together and to systematise the facts and laws which have been brought to light by all those branches of study which directly or indirectly have man for their object. . . . The physiologist, for example, studies man as a wondrous and complicated human machine. . . . The comparative anatomist and the zoologist compare his structure with that of the other animals. . . . The psychologist studies the mind of man . . . and ever aims at the solution of the greatest of problems —whence and what is mind. . . . The historian collects and arranges the facts of man's progress in recent times. . . . The geographer determines the localities of the various races that now inhabit the earth. . . . The archaeologist seeks by studying the remains of man and his works to supplement written history. . . . The geologist extends this kind of knowledge to a still earlier epoch . . . by proving that man inhabited Europe at so remote a period that the very contour of its surface was materially different from what it now is. . . . The philologist devotes himself to the study of human speech and through it seeks to trace the chief migrations of nations. . . . And lastly the phrenologist and craniologist have created special studies out of the study of the human brain and skull.

These various studies have hitherto been pursued separately, with great division of labour and no combination of results. Our object as anthropologists is to . . . combine and generalise the whole of the information thus obtained. The anthropologist must ever bear in mind that, as the object of his study is man, nothing pertaining to or characteristic of man can be unworthy of his attention.'

The new Department continued without further change till 1883. Its presidents

[1] Report for 1866, p. 93.

after Tylor in 1869 were, in turn, John Evans, William Turner, A. Lane Fox (Pitt-Rivers), John Beddoe, Sir W. R. Wilde, G. Rolleston, A. R. Wallace (again), F. Galton, T. H. Huxley, E. B. Tylor (again), F. W. Rudler, W. H. Flower, W. Boyd Dawkins and W. Pengelly. These names give a fair idea of the scope the Department covered : ethnology, physical anthropology, and prehistory were its main interests ; there was also, much British archaeology of later periods (but little foreign) and some folklore. Dialect and linguistics still appear, though more and more rarely. Social anthropology in the modern sense scarcely yet appears : the science was only then being brought to birth. The average number of papers read annually was between 25 and 30 and their general trend depended on the interests of those who came regularly and offered communications. There must have been little or no attempt to plan a programme, or even, perhaps, to attract new speakers. Names such as Park Harrison, J. Beddoe, Miss A. W. Buckland, Hyde Clarke, J. S. Phené, C. S. Wake, General Pitt Rivers, and even R. G. Latham, who had been a regular speaker since 1844 and endeavoured to keep the linguistic interest of the Department alive, occur frequently. John Evans, Thomas Wright, Roach Smith and others, who were primarily archaeologists, do not seem to have been assiduous devotees, judging from the yearly programmes.

At length in 1884, by resolution of Council taken after the Southport meeting in 1883, our subject was divorced from Section D (Biology) and the present Section H (Anthropology) was founded. The meeting that year was at Montreal. E. B. Tylor was once more in the Anthropological chair and the Section's secretaries were G. W. Bloxam and W. Hurst. At this time (1883) Tylor also became the first Professor of Anthropology at Oxford and he was an obvious choice as President of the new Section. But we may guess (when we remember that he was also the first President of Section D's Anthropological Department in 1869) that he was one of the prime movers in persuading the Association thus to recognise our subject as worthy of Sectional status. That it was so worthy we can have no doubt. Yet it is a curious fact that in the early 1880's anthropologists were perhaps less active in the Association than they had been in the fifteen years previous to 1880. In 1882, for instance, Section D's Department of Anthropology could only muster 16 papers for its meeting at Southampton.

That Council ignored (if indeed it even noticed) this temporary trend was justified. The Section developed rapidly and a glance through the annual programmes—I will not weary you with details—makes it clear that the Sectional Committee yearly tried to obtain as wide a spread as possible over all the fields that anthropology might reasonably be expected to cover. The list of presidents up to 1900 included such names as F. Galton, A. H. Sayce, John and Arthur Evans, F. Max Muller, R. Munro, W. Turner, W. H. Flower, Flinders Petrie, Hercules Read and John Rhys ; from 1900 to 1930 it included A. C. Haddon, H. Balfour, D. G. Hogarth, W. Ridgeway, J. L. Myres, W. H. R. Rivers, G. Elliot Smith, C. G. Seligman, R. R. Marett, Arthur Keith, Karl Pearson, John Abercromby, H. J. Fleure and H. S. Harrison. These names confirm the Section's breadth of interest and the point needs no further emphasis.

From 1884 the Section has continued without change until to-day, save that, in 1947, its title was changed from 'Anthropology' to 'Anthropology and Archaeology.'

So far I have said little about the other main activity of anthropology within the British Association—the Research Reports and Committees, and grants for research, and I must now pause a moment to consider how it developed.

From the earliest days of the British Association a series of ' Reports on the State of Science ' were prepared annually by individuals or by Committees for discussion at the ensuing meeting, and printed in the annual proceedings of the Association. These still continue as Committee reports and it is normally only through a Committee that grants for research can be made by the Association. The first report I can trace which embraces anthropology is that prepared by the committee set up in 1839, which resulted, as I have already mentioned, in the issue of a questionnaire to be used by those who came into contact with native races. There was not another anthropological report

until 1851, when the Rev. W. Donaldson prepared one on 'Two Unsolved Problems in Indo-Germanic Philology.' This may sound somewhat circumscribed in the ears of modern adherents of the Section, but part of his first paragraph is worth citing [1] as it is the first public statement I can trace explaining why ethnography (i.e. anthropology) should be included in the Association's programmes.

'It is only recently that it [sc. the science of ethnography] has found a place among the subjects of inquiry suggested to the British Association, and until the present year it has been entrusted to a Sub-section only. But if we estimate its value properly and consider the diversified range of study which it implies and the vast number of labourers who are contributing in different ways to lay the foundations of this great edifice, we may fairly plead for a recognition of its right to a foremost place among the sciences which it is the design of this Association to advance. At any rate there is no branch of study which is more likely to profit by the retrospective surveys for which an annual meeting like the present furnishes so good an opportunity. For, while no science is more steadily progressive than ethnography and while none more rapidly accumulates the materials of induction, its encyclopedic range and the want of communication between the many active minds engaged upon it, especially necessitate a periodic report of its existing state, such as may suffice to indicate what has been really effected, and to point out the objects to which the attention of inquirers may still be most profitably directed.'

The next report, in 1852, was that which led to the second preliminary edition of what was to become *Notes and Queries*. It is entitled [2] 'A Manual of Ethnological Enquiry: being a series of questions concerning the human race . . . and adapted for the use of travellers and others in studying the varieties of man.' Its sub-heads were: 'Physical characters; language; grammar; individual and family life; buildings and monuments; works of art; domestic animals; government and laws; geography and statistics; social relations; religion and superstitions.' It is clearly therefore a proper precursor of its long line of successors. Small grants of money totalling over £40 in all were paid out at various times between 1841 and 1855 for items termed either 'Races of Men' or 'Ethnological Enquiries' and these must all have been concerned with the preparation, printing and distribution of this manual or its predecessor of 1843.

Thereafter for thirteen years reports and grants for anthropological work were rare. Those that are recorded, such as reports on 'A Series of Skulls from Nepal' in 1859 and on 'Ethnoclimatology, or the Acclimatization of Man' and 'The Psychical and Physical Characteristics of the Andaman Islanders' in 1861 need not detain us. There was also a grant of £20 paid out in 1861 for excavations at Uriconium (Wroxeter).

Then in 1865 the picture changes just at the time when the new Anthropological Department in Section D was being formed. Anthropological reports and grants now become a yearly feature. The most important are the series of no fewer than 16 reports and grants between 1865 and 1880 relating to the excavations in Kent's Cavern under W. Pengelly's direction. But there were also during these years reports and grants for work on Maltese, Gibraltar and Borneo caves (basically for the study of prehistoric animal remains), and on Settle, Tenby and Loughton caves and on caves in Ireland; there were reports and grants, too, for work by the Palestine Exploration Fund (1866, etc.), for the first substantive edition of *Notes and Queries in Anthropology*, edited by Col. Lane Fox (Pitt-Rivers) (1873–6), for British Earthworks Research (1877–8), and for Anthropometric and Racial Research (1877–1884).

By 1884, therefore, the British Association had already helped greatly in leading and financing anthropological research, and this activity has continued without break until the present day. I need only mention money given for numerous excavations in Britain and abroad, e.g. Oldbury (1890) Glastonbury (1894 ff.), Silchester (1897 ff.), Crete (1901 ff.), Neolithic sites in Greece (1910), British Somaliland (1930), Galilee (1930), and

[1] Report for 1851, p. 138.
[2] Report 1852, p. 243 ff.

many others; for ethnological research in Canada (1898 ff.), the Torres Straits (1898), and Malaya (1900); for work on the Age of Stone Circles (1902 ff.), and the Distribution of Bronze Age Implements (1930 ff.). Unfortunately, the grants are normally not so large now as they once were, but the principle remains and flourishes.

I hope I have made it clear without going into too great detail that Section H, founded for the 1884 meeting and now 72 years old, has, in its annual programmes and in the activities of its Research Committees, spread its net widely over a field of study embracing prehistoric and historic archaeology (both British and foreign), physical and social anthropology, ethnology (both of primitive and advanced cultures), folk-lore, and comparative religion. It has not, of course, year by year succeeded in including something of all these in its programmes; but its Presidents have from time to time been drawn from the leading scholars in the many divisions of our subject, and its Recorders have always, I think, endeavoured to ensure that each of these subjects has been included in the programme—if not every year, then at least at frequent intervals.

Merely to enumerate these branches of anthropology, as I have done, gives a view of the breadth of our interests. But their great expanse is, if you think of them in detail, even more complicated and widespread. Take archaeology! A Sectional programme in any one year could be devoted purely and simply to archaeology and yet it would perforce leave many branches of that study untouched. For archaeology embraces the past cultures and early artefacts of the whole world, and its interests have now become so complicated and widespread that it is impossible to collect together at any one meeting exponents of all its branches, even if there were time, when they were together, to allow each group a place in the programme. The archaeology of the Old World alone is broad enough and is ever getting broader, now that the prehistory of darkest Africa and of eastern Asia are being more and more studied, and are taking their places beside the older disciplines of palaeontology, Egyptology, Assyriology and the Greek and Roman and other European cultures.

Or take social anthropology, ethnology and comparative religion! The days when a Frazer could by himself cover expertly in his *Golden Bough* something of all these studies from all over the world are long past. To-day—and we may be sure that Section H itself has taken its fair share in bringing this about—the detailed research which is being undertaken by students of all lands into the social habits and customs and laws of primitive peoples throughout the world, the voluminous and detailed literature that has been written about primitive and comparative religion, and the important research into the arts and crafts of backward, and indeed not always so backward, peoples, provide a field of immense scope. Each successive Recorder of this Section pales, as I know full well, before the task of even touching the fringes of it in his programmes from year to year.

And even now I have left out physical anthropology and folk-life and folk-lore from my brief survey. The first of these embraces not only the story of man's descent and evolution over a million years or more (and Professor Sir Wilfrid Le Gros Clark in his fascinating Evening Discourse last year gave some idea of the complications of such a study); it has from the start also been held to cover the comparative study of modern man's physical types and physiological characteristics, as is emphasised—if such emphasis were needed—by the election of Dr. A. E. Mourant, one of our leading authorities on blood-groups, to succeed me in this office.

At the same time the joint subject of folk-life and folk-lore, though it overlaps with comparative religion at one end of the scale and with primitive technology (a branch of ethnology) at the other, does in fact embrace as well much that is *sui generis* and cannot be covered by any other anthropological label. I need only mention the papers at Belfast in 1952 on 'New Zealand Children's Games' and at Oxford in 1954 on 'The Natural History of English Oral Lore' (which dealt largely with children's rhymes) to indicate how far our Section spreads into the field of folk-life and lore.

Our Section, indeed, true to its name

' Anthropology,' echoes year by year with the words of Terence ' Homo sum humani nil a me alienum puto ' or, as Pope loosely translated it, ' The proper study of mankind is man.' That it has recently added the word ' Archaeology ' to its title seems not only unnecessary but misguided. Was this change made because the great growth of archaeology in the present century and the *amour propre* of its enthusiastic exponents caused them to feel that anthropology, which has in popular parlance come more and more to be narrowed down in usage to cover the two branches of social and physical anthropology, was not an umbrella under which it was dignified for them to shelter ? If so, they had not forgotten their Greek ? Surely anthropology,—the study of ἄνθρωπος, the human race—is a large enough umbrella for us all, whether we be archaeologists or folk-lorists or students of physical or social anthropology, to shelter under ? Ethnologists have never shown the same feeling of almost dislike for the term Anthropologist.

I know that this view of mine may not prove pleasing to many of my hearers, who, as archaeologists excavating important Near Eastern cultures, for example, may not like to think they have any connexion with those who study the social habits of primitive tribes, or who, as social anthropologists whose laboratory specimens are real living people, look with some disdain at those who play at jigsaw with fragmentary skeletons or potsherds, crying ' Can these bones live ? '

It is, of course, only human to be mainly interested in one's own branch of this vast subject of ours, and all specialists are today getting more and more narrow in their fields of specialisation. Those who try to do more, and to cover several branches of the subject at once, run the risk of developing a superficial knowledge of much, rather than a detailed knowledge of very little. The ' risk,' I said : but might not a little superficiality, in the better sense of that term, be good for some of us, and might it not help us to understand our own little branch of the subject better if we had more acquaintance with what others are doing in related fields of study ?

Now the Association as a whole exists, above all else, as a medium for bringing scholars and students of many sciences into direct touch with one another, and its more constant adherents find this one of the great attractions of its annual meetings. Should not we of Section H take a similar view within our own smaller orbit ? Indeed, do not some of us already take such a view ? When we need, or wish, to encourage our specialist tendencies we have our specialist conferences which we can attend—our meetings of the Prehistoric Society or of the Royal Archaeological Institute or of the conference of the Association of Social Anthropologists, or what you will. Is it right to wish the British Association to become a substitute for one of these, or to attempt to make it such ? I venture to think it is not ; and that it would defeat the whole object of the Association if the annual programme of Section H became just one more specialist conference.

I have indeed heard it said more than once by otherwise well-meaning and intelligent students in our several branches that it is not worth while to come to the British Association meetings as there is so little in any one programme that bears on their own circumscribed speciality. How misguided is such a view ! Do they never discuss their subjects with, or discuss other subjects with, colleagues and friends who are not of the same discipline as themselves ? And if they do this at other times, why should they not try the same medicine for their ills of specialisation during a meeting of the Association ?

But I would like to put the matter on a broader plane than that. Anthropology as the study of man's thoughts and being and activities in the past and in the present—such is perhaps a simplified definition of our subject—touches on so many other sciences and disciplines that we as anthropologists cannot afford not to be mindful of what our brother scientists in other fields outside Section H are doing and thinking, and, more urgently still, of what they can do to help us.

There is no one sectional activity within the Association with which anthropology does not have contact at some time or another. Let me elaborate this statement a bit, which at first sight may seem an exaggeration, though it was sufficiently plain to A. R. Wallace as far back as 1866,

as my quotation from his Presidential Address at that meeting makes evident.

We look to physicists and chemists for help in spectrographic and other analyses, as Dr. Hall demonstrated to us last year at Oxford and as was previously proved to us at the joint session we held with Section A (Mathematics and Physics) at Birmingham on Carbon-14 research, and in Dr. Oakley's talks in recent years on fluorine and nitrogen content of bones—techniques which have been so aptly and successfully applied in recent studies of the Piltdown material. New ways in which physical and chemical scientific methods may be applied in anthropological research will surely be developed in the future by Dr. Hall and others in the new archaeological research laboratory at Oxford, and elsewhere, and thus our contact with Sections A (Mathematics and Physics) and B (Chemistry) will be increased still further.

Our prehistorians are never far removed from Section C (Geology) in their study of geology and palaeontology and it is a matter for surprise and regret that we have not had a joint session with the geologists in recent years: our predecessors were not so unwise. But, apart from prehistory, our Sectional studies can make contact with geology and mineralogy in many ways and derive much help from them. I need only mention the recent work on the petrology of stone axes, which has revealed so much about neolithic trade in this country by identifying the sources and even at times the quarries from which the axes were derived, showing what great distances some of them travelled. But stone has been used by man for many things besides axes and in each case the petrological study of the stones he used can be most revealing, as Mr. Jope pointed out to us at Belfast in regard to building-stones in Roman and medieval England.

Sections D (Zoology) and K (Botany), our former colleagues when we formed part of the Association's Biological Section from 1869 to 1883 are no less closely allied to us still and with them we may also consider Section M (Agriculture). Without contact with the zoologists we cannot hope to recreate in our minds the animal ecology of past ages and the history of the domestication and use of animals, not to mention the constant interchange required between zoologists and our physical anthropologists in connexion with the rise and development of *homo sapiens*. And just as zoological help is essential in recreating the animal side of early ecology, so are botany, forestry and agriculture equally essential in recreating the plant and tree ecology of our ancestors and in ascertaining their knowledge and utilisation of soils and crops at different periods and in different climes. Man has always been the slave of his botanical and climatic environment, however much from time to time he may have tamed portions of it for his own uses—plants as food, trees for building, and so on. A great part, too, of our dating of the earliest ages of man's existence is founded on palaeobotanical studies, while the identification of the crops and trees that man from time to time has used and known in different places is of great importance in reconstructing his economy and standard of living. All this has been pointed out to us by Dr. Godwin's paper at Dundee in 1947 on the principles and practice of dating archaeological remains by pollen analysis, by Dr. Steensberg in his paper at Edinburgh in 1951 on the use of analysis of charred grain and wild seeds, as well as pollen, in fixing the archaeological date of a climatic change which took place in Denmark and elsewhere about 1300, as well as by Dr. Mitchell at Belfast in 1952 and by Dr. Dimbleby in Oxford last year.

With Section E (Geography) we are perhaps more allied and in more constant touch than with any other Section, as our sojourn as ethnologists with them in the years between 1851 and 1869 long ago demonstrated. Our studies touch theirs at almost every point, whether we be archaeologists or social anthropologists or students of ethnology and primitive technology, for so much of what man has accomplished at any one time or place is dependent on the geographical and climatic milieu in which he lived. The fact needs no further elaboration and the great interest which geographers from their side manifest in anthropology in its several branches indicates that the help can also flourish in the reverse direction.

So far, the other Sections I have mentioned seem perhaps to have more in

common with the archaeologists and physical anthropologists in our midst than with those of us who study social anthropology. With Section F (Economics) it is, of course, otherwise. Allied as economics is to sociology and the study of man as a social being, it has a bearing not only on the social anthropology of living races, but also upon the social and economic achievements of the more civilised past cultures, which can often be elucidated and illustrated by present economic thought and practice.

I come next to Section G (Engineering) which—far removed as it may seem to be from what we anthropologists consider and care for—may yet meet us at more than one point. Even the ancients engineered roads and bridges and used quite elaborate instruments and measurements in architecture and other civil and military engineering. Stonehenge and the Great Pyramid are admitted by all to have been two of the world's great engineering feats and even earlier than that the construction of an elaborate flint mine or one of the neolithic coracles or canoes, which carried men vast distances across oceans or up rivers, represented in its own way, and for its time, a feat of engineering comparable to-day with the digging of a major coal-mine or the building of a Dreadnought or the Queen Elizabeth.

That Section I (Physiology) can overlap with physical anthropology and Section J (Psychology) with social anthropology needs no argument or explanation ; but each can help other branches of our subject too, and it is fair to claim that the story of ancient medical practices, and even of the herbal remedies of primitive tribes and of our modern peasant, is of as much interest to the student of modern medicine as is the study of primitive thought to one who seeks to fathom the psychological vagaries and experiences of allegedly superior modern men and women.

And, finally, Section L (Education). That modern educational studies and theories may experience any overlap with anthropology may not seem at first sight evident. Yet man has at all stages in his history needed to educate his young, and the education of primitive peoples to-day cannot be far different from, and may therefore be used to elucidate, that practised by people at a similar stage of culture in the past. So, too, the educational methods of the great civilisations of history, notably those of Greece and Rome, have had such an effect on the highest grade of modern education that there must always be a strong connexion between our two Sections.

If in this brief summary I have seemed to emphasise more how the studies of these other Sections can help us, it is not that I am unmindful that we equally can often help them. Indeed our work on the ancient and the more primitive can not only be of academic interest to, but can often be of direct service to, those studying various aspects of their own scientific discipline. This is more true perhaps in relation to biological than to physical studies. Examples will spring to your minds without any prompting from me in showing how zoologists or botanists or physiologists can clarify their modern problems by recourse to what has been shown to have happened in the past, or to skeletal and plant remains collected on archaeological excavations or from the dwellings and homesteads of primitive peoples. But even the physicist and the engineer need not despise our anthropological discipline, and—as many of them would readily admit—they can often derive both profit and pleasure from learning how the best minds in past ages have solved problems which are concerning them on a different plane to-day. Nay more : they, too, can even learn facts that are of value to-day from archaeological discoveries. In his talk at Belfast on 'Antiquities, Ancient Monuments, and Research on Materials,' Mr. Biek showed how some iron objects of medieval or earlier date found at Hungate in York were quite uncorroded and, on analysis, the soil surrounding them proved to be unusually rich in phosphate and tannate, which had apparently combined to protect the iron. Mr. Biek has also studied the same phenomenon more recently in the Roman well at Chew Stoke near Bristol, and I understand that these discoveries are already proving to be of value in protecting buried pipe-lines to-day.[1]

What then must we conclude from our glance at the Section's past and at the

[1] *Advancement of Science* IX, p. 436 ; and verbal information kindly supplied by Mr. Biek.

contacts we can now discern between anthropology and the other disciplines that lie in the Association's orbit ?

In the first place it is clear that anthropology, the study of man, is certainly by its very nature a microcosm of the Association as a whole, and we might perhaps infer that there is no need for a special Section to cover it, as it can exist within each and every other Section of the Association. But we would, of course, be misguided in deciding thus, for to scatter anthropology amongst all the other Sections would not give the subject the prominence or coordination that its importance warrants. There is, in truth, full room for our Section, and it has its own very special part to play within the Association.

That it may not be playing that part properly to-day is not so much the fault of the Section in the abstract, as of its more recent organisers. They, if I may say so (as I was myself for five years recently its Recorder !), have perhaps often failed to consider the Association's own purpose when devising its programme from year to year. For one thing is crystal-clear. Merely to plan annual programmes which will attract and satisfy the die-hard specialists in even some, let alone all, of the various branches of our subject, is to court failure and to make sure of attracting neither the specialists we are aiming at, nor the more broad-minded students in our own and other fields of study. Our subject is too widespread for that.

Perhaps I would be going too far if I demanded that we should eschew all detailed and specialist papers. But at least let us work to a plan : and the plan that I would champion is to take advantage of the existence of other Sections to arrange programmes annually which will demonstrate the overlap between Section H and all the other disciplines represented in the Association. It will not be easy and will require much thought and effort and the most careful choice of subjects and speakers. But surely it is worth trying.

A concerted effort towards this end, though on a minor scale, has been attempted in recent years in the sessions on *Scientific techniques as aids to archaeology*, which took place, for example, at Edinburgh in 1951, at Belfast in 1952 and at Oxford in 1954 ; and there have also been individual papers in the Section's programme from time to time on subjects such as pollen analysis and stone-axe petrology, as well as joint sessions like the one with Section A at Birmingham in 1950 on Carbon-14 research and another with Sections D and I at Brighton in 1948 on human blood-groups. These certainly have pointed the way : but they are not sufficient and I would venture to suggest that if more could be done in this direction it would give pleasure and satisfaction to many and would lift Section H out of the slough of uncoordinated specialisation in which it has recently tended to get engulfed.

It is this tendency, I believe, which has caused not a few anthropologists of various persuasions to complain that the Section's spread is too vast and that it were better to divide the Section into two or more parts, so that room could be found for an even greater number of specialist speakers year by year. I dare say that the same complaint could be made on similar grounds about the content of the programmes of other Sections ! But is it a fair complaint having regard to the purpose of this Association for the Advancement of Science ? I think not.

It is, indeed, my belief that by this time I have answered in the affirmative the question posed in the title of this address and shown that anthropology is a scientific unity. But I trust that I have gone farther and shown also that, properly used, our Sectional programme could become a unifying influence within the Association as a whole. Other Sections—particularly Section E (Geography), which also touches many other Sections closely—will, I hope, forgive me if that appears to be too presumptuous a claim. To me it seems a fair and proper one. That being so, let us hear no more talk of splitting our Section into two or more parts. Rather must we hold fast to our unity and invite our successors in office to plan ahead and devise programmes which will not only retain year by year the interest of all broad-minded anthropologists, but will attract and not repel adherents of other disciplines within the Association. That this can be done is clear ; but that it will need much effort and forethought is no less evident. It is indeed both a challenge and an opportunity.

THE SENSORY PATHWAY

Address by

DR. R. S. CREED

PRESIDENT OF SECTION I

IT has long been known that the nervous pathways in higher animals from receptors or sense-organs to the sensorium are never direct and uninterrupted. On the contrary they dip at intervals into the grey matter of the central nervous system, and where they do so the nerve fibres which have so far been conducting the message terminate to form functional connexions or synapses with a new set of nerve cells. Processes of these cells constitute the fibres along which the message passes to the next relay station. Many writers have also remarked on the anatomical similarity in the organisation of the pathways from different end-organs. Thus the nerve fibres supplying the diverse endings in the skin, some being encapsulated structures of varying complexity and others consisting merely of naked branchings of the terminal nerve fibre, are all processes of cell-bodies lying in the dorsal root ganglia. They subserve sensations of touch, warmth, cold, and pain. The central processes of these root ganglian cells enter the spinal cord and end in one of two ways on their path to the sensorium. Some almost immediately enter the grey matter and make synaptic connexion with cells in the posterior horn, whence new fibres arise which cross to the opposite side of the cord to ascend to the optic thalamus at the base of the cerebral hemispheres. Others pass upwards in the posterior columns of white matter of the same side and do not end until they reach the gracile and cuneate nuclei at the very top of the spinal cord. From the cells of these nuclei, relaying fibres again cross the mid-line to end in the thalamus on the opposite side. From the cells of the thalamus a third set of nerve fibres carry impulses to the cerebral cortex, and it is only when the excitation reaches this level that consciousness is aroused and sensation experienced. The second of these pathways is also the one by which messages pass from endings in muscles and joints to make us aware of the position and the movements of our limbs.

From the cochlea of the ear, fibres which are processes of peripheral nerve cells in the spiral ganglion of Corti pass into the hind-brain and there make synapses with the cells of the cochlear nuclei. From these nuclei arise fibres which ascend in the lateral fillet of the same or the opposite side to end in the medial geniculate body of the thalamus. Thence the auditory radiations relay impulses to the temporal lobe of the cerebral cortex of the same side.

For vision a similar three-neurone route exists : the retinal bipolar cells connect the receptor rods and cones with the ganglion cells of the retina ; processes of the ganglion cells constitute the nerve fibres of the optic nerve and optic tract terminating in the lateral geniculate body ; and the third stage is the optic radiation running from the lateral geniculate body to the occipital lobe of the cortex.

This brief summary of the anatomy of the sensory pathways is incomplete and in some respects an over-simplification. In the case of pain, for instance, there is evidence that even in the spinal cord the path may sometimes dip repeatedly into grey matter in its upward course, with the result that surgical division of the long tract of white matter which is the main pathway may not always give permanent relief of pain originating lower down. These short relays then involve a large number of successive synapses between receptor and sensorium. So too in the pathway for hearing there is reason to believe that there are often more than two

synapses between the fibres of the cochlear nerve and the fibres which reach the auditory cortex. Nor must it be assumed that the sensorium (Boethius), i.e. the place where nervous activity affects consciousness, is invariably the cerebral cortex. Sensations of pain in particular seem to be aroused by activity in the thalamus rather than in the cortex. Even in so highly intellectualised a sense as vision it seems certain that lower mammals, though not man, can not only discriminate between light and darkness but also avoid obstacles in their way after extirpation of the so-called visual cortex. In other words, they are not totally blind and we must not regard the cerebral cortex as synonymous with the sensorium. The crossing of spinal pathways to the thalamic nuclei of the opposite side is probably never complete, with the result that activity can often be recorded in the higher centres on both sides after appropriate stimulation of skin or deep structures in a single limb.

However, there can be little doubt that the picture I have sketched is accurate in its main outlines. The remainder of this address will be devoted to a discussion of the significance of so complex a pathway and will be expressed in as simple language as possible so that it may be comprehensible to those who are not physiologists. Indeed the professional physiologist will not hear much that is new to him, but that fact calls for no apology. The most striking feature so far brought out is the existence of interruptions at at least two points in the pathway. What is the reason for these breaks? Why is there not a direct pathway for through traffic from receptor to brain?

The nervous system has often been compared with a network of telephone lines and the grey matter with telephone exchanges where messages can be shunted or diverted in various directions. But this analogy, though useful, can mislead. As we know from study of reflex action, much more may happen in grey matter than mere switching of excitation along certain channels; inhibition, temporal and spatial facilitation, algebraical summation, after-discharge, occlusion, rebound, and fatigue are the technical terms applied to some of these happenings. They may be expected to occur just as much on purely sensory pathways as on the receptor-motor channels of reflex action. Moreover the telephone analogy might suggest that a function of the synaptic interruptions is to act like the amplifying relays which must be installed at regular intervals in a long-distance telephone line. In their absence the signals would become too faint to be recorded at the receiving end. Nerve fibres, however, behave quite differently. It has been known for fifty years that the messages they convey consist of discrete nerve impulses which derive their energy, not from the stimulus which excited them, but from the activity of the living fibre in which they travel. The stimulus acts like a trigger firing the impulse, a self-propagated disturbance that traverses an indefinite length of nerve fibre without decrement. If a single fibre ran the whole way from receptor to sensorium the nerve impulse would arrive with the same energy as if it passed through intermediate relay stations on the way. We must therefore look for other explanations of the interrupted pathway.

Formerly the physiology of the senses was mainly studied by comparing the stimuli applied to receptors on the one hand with the sensations experienced by a conscious subject on the other. Confirmatory evidence could be obtained in other ways such as from the behaviour of animals. Indeed Pavlov's studies of 'conditioned reflexes' provided a valuable technique for investigating the abilities of lower animals to discriminate between qualitatively different stimuli and shewed, for example, that dogs appeared to be colour-blind but could distinguish better than man between sounds of slightly different pitch. But the subjective experiences described by man were by far the most important data on which the physiologist could theorise. Great advances in knowledge were made and are still being made by such methods. It is difficult to believe that in the foreseeable future they will ever be dispensed with. They have, however, the obvious disadvantages and difficulties inseparable from subjective introspection. They are liable to be influenced by presupposition and suggestion. Accurate description of unusual sensations which have never before

been experienced or analysed requires frequent repetition under strictly controlled conditions by a number of unprejudiced observers whose descriptions can never be expected to be identical. The relation between stimulus and sensation involves comparison of what can be measured by fairly accurate physical methods with what is subjectively assessed no more accurately than by saying 'less' or 'more' or 'qualitatively different'. Nor does sensory experience provide direct evidence of the anatomical site in the long and complicated pathway at which subjectively experienced change in sensation has been caused. For all these reasons objective studies of the actual physical events at various levels will clearly give most valuable information.

Until about thirty years ago detailed knowledge of the properties of nerve fibre was chiefly derived from electrical records of action currents in large nerve trunks containing many hundred individual fibres and from study of the responses of muscle to electrical stimulation of the nerve fibres supplying it. It was only by analogy that it could be assumed that the messages carried inward to the central nervous system from sensory endings were similar in nature to, and obeyed the same laws as, those carried outward to muscles. Adrian's pioneer work on nerve action-potentials by the use of valve amplification proved this assumption to be correct and he obtained the first records of the nerve impulses set up in a single sensory nerve fibre when its endings were appropriately stimulated. The frequency of the impulses was found to vary with the intensity of the stimulus but there was no change in the size of the individual impulses. Intensity discrimination must therefore depend only upon the number of nerve fibres which are excited and upon the frequency of the impulses which they transmit. This work has been extended by Adrian and others to every sense-organ in the body and to almost every type of receptor. All behave similarly as judged by recordings from single fibres or single nerve cells. In some cases, notably the auditory pathways in the mid-brain, the optic nerve, and the thalamus, responses of single units further removed from the sense-organ than the first relay in the ascending path have been studied when the natural stimuli are applied to the receptors. The messages transmitted all appear to be alike, so that there can be little doubt of the validity of Johannes Müller's Law of Specific Energies of the Senses according to which the quality or modality of the sensation experienced depends solely on the anatomical pathway by which the excitation reaches the sensorium. Not only is it immaterial how that excitation was produced, i.e. whether by the natural stimulus or by some unusual means, but the nature of the message, a succession of nerve impulses, is the same in all pathways.

Adrian again has been the pioneer in study of the electrical changes in the cortex itself, by the technique of electro-encephalography in intact man and by recordings from the exposed cortex in animals. By these methods information has been obtained about the cortical sites at which activity occurs as a result of peripheral stimulation and about the character of that activity. Much more is likely to be learnt in the next few years when improved technique has made it easier to obtain records of the activity of single units in this situation. Meanwhile let us return to the lower levels of the sensory pathway.

In the skin it is well established that there is considerable overlap in the areas of distribution both of nerve trunks and also of individual nerve fibres. Consequently, when a cutaneous nerve or a dorsal spinal root is severed there is an intermediate zone of diminished or altered sensibility between the completely insensitive central area and the surrounding skin where sensitivity is normal. Before the injury this zone must therefore have been supplied by nerve fibres from both the severed trunk and neighbouring trunks. The sensory properties of such intermediate zones in man after section of cutaneous nerves have been investigated by a number of workers and the facts are not much disputed. On this occasion it would serve no useful purpose to revive the controversies to which the interpretation of these findings has given rise. But it is perhaps worth while to mention a curious and little known difference between the results of cutting a cutaneous nerve on

the one hand and a dorsal root on the other. In the former case the area insensitive to light touch is usually considerably more extensive than the area insensitive to pain. The pain evoked in the intermediate zone is indeed much more disagreeable than pain evoked from normal skin though it may require a stronger stimulus to produce it. It is clear then that pain fibres from neighbouring cutaneous nerves overlap with one another more than do fibres subserving touch. The reverse is true of adjacent spinal roots. Sherrington (1901) in the monkey and Head (1905) in man independently discovered that after section of spinal roots there is an intermediate zone responsive to tactile stimuli but insensitive to pain.[1]

Overlap of the area of distribution of individual afferent fibres is almost certainly of widespread occurrence in the skin and in other sense-organs such as ear and eye. This has sometimes been thought to raise theoretical difficulties, in particular about the neurological basis of localization of stimuli. In the case of skin pain, for example, there is reason to believe that the peripheral ramifications of a single afferent fibre may be distributed over a considerable area. Does this mean that the site of application of a stimulus acting only on pain-endings could not be identified more accurately than to within this area? It means nothing of the kind if we take into account other pain-endings derived from other afferent fibres distributed to parts of the same area. Suppose, for instance, that the middle of the area is supplied only by fibre A, but that fibres B, C, D, and E each supply peripheral parts of the area. Let us then assume that there is a mechanism in the central nervous system which responds differently according as impulses come in along fibre A alone, or along fibre A and fibre B together, and so on. In this way five discriminable types of message may be sent to the sensorium from differet parts of the area of skin innervated by fibre A. Nor is there any *a priori* reason why still finer localisation of the site of the stimulus should not be achieved. For it is plausible to suppose that in the region of overlap between A and B the relative density of the endings of the two fibres will vary, and hence that the relative frequency at which impulses are fired along them will also vary with the place of stimulation. The central nervous system will thus receive different types of message from within this one sub-area. Similar arguments can be advanced about comparable problems arising from overlap in cochlea and in retina.

In most sense-organs there are far more receptors than there are nerve fibres running from them. An obvious example is the human eye which contains over 120 million rods and cones connected with the brain by about a million fibres in the optic nerve. In the organ of smell again there are some 80 million sensory cells on each side but there are only about 4,000 'glomeruli' in each olfactory bulb and perhaps 70,000 fibres in each olfactory tract. This convergence of many paths on to a single common path is of great significance. Various forms of interaction between the messages coming in may occur at these relay stations. The simplest is that they will reinforce one another by a process of facilitation or summation. The train of impulses sent onward will then be at a higher frequency than that of the incoming ones. Thus we may explain the phenomenon of areal summation which is often such a striking feature of sensations aroused from skin and from retina.

Definite objective evidence of nervous interaction at synaptic levels in the retina was obtained by Adrian and Matthews when they recorded action-potentials from the optic nerve of a conger eel on stimulating its retina with light. The interaction took various forms, especially striking being facilitation between neighbouring areas of retina. Strychnine increased the effect. So far as sensations are concerned, water which is quite bearable when only the tip of one finger is dipped into it may feel intolerably hot when the whole hand is immersed. A large cold object pressed on the skin feels much colder than a small

[1] An even more remarkable observation of Head in an intelligent patient whose 5th, 6th, and 7th cervical dorsal roots had been severed by Horsley for the relief of pain was that in the intermediate zone of skin the contact but not the coldness of ice was readily appreciated, whereas an object at 25° C. was at once pronounced cool. Similiarly, warm but not hot objects aroused temperature sensations. These findings are perhaps more likely to be related to the receptor mechanism than to the sensory pathway and will therefore not be further discussed.

object at the same temperature. Quantitative analysis of such effects can be made in vision. A large patch of light in an otherwise uniformly dark visual field is visible at a much lower intensity of illumination than is a small patch. If the retinal image of the patch falls on fewer than about 4,500 rods and cones, the relation is such that the product of area and brightness is constant at the threshold of visibility (Riccò's Law). This must mean that the threshold for sensation is higher than the threshold for excitation of the individual receptors. No sensation is aroused unless the effects of stimulation on a number of receptors are summated, and the site in the pathway where the summation occurs must be nerve cells with each of which many afferent fibres form synaptic connexions. If we turn from the study of sensation in man to the study of reflex action in the spinal animal, we can readily find comparable examples of spatial summation of the effects of subthreshold stimulation in grey matter. Here too the only possible explanation is convergence of several pathways towards a final common path, and the threshold of excitation for the individual receptors must be lower than that for the reflex response.

One of Sherrington's most important contributions to our understanding of reflex action was his demonstrations that central inhibition is as characteristic and essential a feature as is central excitation. We still do not fully understand its mechanism but there is no doubt that when a stream of ingoing impulses enters the spinal cord they excite some nerve cells and inhibit discharge of impulses from others. One group of muscles contracts; another relaxes and becomes unresponsive to reflex influences which would previously have made it contract. Eccles's work in the last few years with microelectrodes inserted into the cell bodies of motor nerve cells in the spinal cord provide additional evidence that the arrival of impulses at some synapses applied to the surface of these cells causes them to discharge impulses to the muscles which they supply, while the arrival of impulses at other synapses inhibits their discharge. In the sensory pathway it is difficult to prove rigorously that inhibition as well as excitation may occur at the relay stations in grey matter, but it would, to say the least, be very surprising if it did not. Moreover there are many observations which suggest that the passage of impulses along some ascending paths may impede or inhibit such passage along others. Head produced evidence from normal man that the threshold for cutaneous pain is raised when temperature sensations are aroused from the same region. In vision the phenomena of colour contrast and brightness contrast suggest inhibition at synapses in either the retina or the brain as their most probable basis. Some of Granit's observations on flicker and on electrical recordings from the eye are hardly explicable except in terms of inhibition. By intensifying contrast, inhibition probably plays a part in visual discrimination of detail. Davis and his collaborators recording action-potentials from the auditory pathway provide further evidence of active inhibition.

A puzzling anatomical feature of the ascending pathway from certain sense-organs is that, after a preliminary convergence, a marked divergence occurs, i.e. the number of nerve fibres increases as the sensorium is approached. In each glomerulus of the olfactory bulb connexion is made with from 10 to 20 mitral cells along whose axones impulses subserving smell pass onwards. Each fibre of the monkey's optic tract forms synapses with 5 or 6 cells in the lateral geniculate body (Le Gros Clark). Each of these cells in turn is connected with, on an average, about 135 cells in the occipital cortex (Chow). Another way of illustrating the same fact in the visual pathway is that, according to Talbot and Marshall, the central region of a monkey's retina is represented by an area 10,000 times as extensive in the visual cortex. Again in cell counts at successive synaptic levels in the auditory pathway of the monkey Chow reports a steady increase amounting to over 100-fold between the spiral ganglion of Corti and the cerebral cortex.

If, to use Pavlov's terminology, we think of the cortex as the *analyser* of sensory messages, we may well ask ourselves how this divergence of pathways assists in the analysis. Clearly detail cannot be manufactured by the cortex out of material

which is undetailed at lower levels. The cortex can only analyse such detail as is presented to it. It can perhaps magnify detail which is already there. An analogy may be found in a newspaper reproduction of a photograph. Slight magnification with a lens may enable us to see a little more detail than with unaided vision. But powerful magnification is worse than useless: the picture merely becomes an arrangement of dots conveying no meaning and representing nothing recognizable. Talbot and Marshall have suggested that the multiplication of paths in the central nervous system may assist in analysis because with 'extensive reciprocal overlap' at successive levels detail may be sharpened; the high lights may become brighter and the shadows darker. Their argument is ingenious and attractive and will not be developed further here. I hope I have not misrepresented it in condensing it into a single sentence.

There are, however, other possibilities which may be worth thinking about. If incoming messages arrived in closely adjacent nerve fibres, detail already implicit in them might on reaching the sensorium become blurred for any of the following three reasons. Electrotonic spread of excitation to neighbouring nerve cells might occur by the potential fields set up. Neurological spread might result from synaptic communications between cells. Chemical transmitters, if there be such at synapses in the central nervous system, might diffuse to affect cells other than those to which the synapses were applied. An obvious objection to these speculative suggestions is that blurring would have already occurred at lower levels. However, in the one case in which definite evidence is available, viz. the visual pathway in monkeys, Le Gros Clark and Glees have described an unusual state of affairs in that only one synapse from an optic tract fibre impinges on each nerve cell in the lateral geniculate body. This arrangement may conceivably prevent unwanted spread of excitation at this level.

Finally, it is worth while to devote a few minutes to the pathways from the two great paired sense-organs of sight and hearing. Both eyes are connected with both cerebral hemispheres in such a way that the right halves of each retina are represented in the right occipital lobe and the left halves in the left. Corresponding parts of the visual fields of the two eyes are thus represented in closely adjacent or almost identical sites in the cortex. A small lesion in this part of the cortex will cause similar patches of blindness in both visual fields. Below this level the anatomical pathways from the two eyes are entirely distinct, but it might be expected that when they do finally converge on the visual cortex they would reinforce one another. This however seems not to be so to any considerable extent. Binocular brightness is only very slightly greater than uniocular, and binocular thresholds are little, if any, lower than uniocular. Each of the twin retino-cerebral apparatuses elaborates its own sensations and percepts almost entirely independently of the other. Binocular fusion is a fusion of these percepts rather than a result of confluence of pathways at what one may call a physiological level.

In the auditory system the picture is very different. Again the pathways ascending from each ear run to both cerebral hemispheres, but in this case there is indubitable evidence that messages from one ear summate with or facilitate those from the other. Sounds appear louder when heard with two ears than with one. The summation is so perfect that in fact such a sound has the same apparent loudness as one of double its physical intensity heard with a single ear (Fletcher and Munson). Threshold determinations shew the same thing. The total energy required in the two ears at the 'binaural threshold' is equal to the energy required in one ear at the 'monaural threshold,' independently of the actual division of energy between the two ears (Hughes). (This, incidentally, provides a good example of the threshold for excitation of receptors being lower than the threshold for sensation; two sounds, each alone below the threshold of sensation, attain that threshold by a process of summation in the central auditory pathways.) In cats and dogs loss of one cochlea causes about the same slight degree of deafness as removal of the cortex on one side. In man unilateral destruction of the auditory cortex causes the same slight degree of

deafness in both ears. At what level the two pathways converge to produce summation of loudness is unknown. That it may be only in the cortex itself is suggested by evidence of a different kind which seems to indicate a considerable degree of independence of the paths. This evidence is that when clicks are sounded not quite simultaneously in the two ears, they fuse and are heard as a single click localized always to the side of the ear which receives the first stimulus. Difference in time of arrival of air waves at the two ears is therefore a factor in the auditory localization of sound sources, and presumably this must also involve a corresponding difference in time of arrival of nerve impulses from the two ears at the cerebral cortex. A remarkable fact about this phenomenon is that a time difference as small as two ten-thousandths of a second is sufficient to produce the effect. That the nerve impulses should retain their time relations so accurately after being relayed through several synapses on their way to the cortex is indeed surprising.

This address has been concerned solely with ruminations about what happens, or may be believed to happen, on the way from receptor to sensorium. It is a cursory review of this limited field intended only to indicate what has so far been established and what may in the future be looked for. It will have served its purpose if it suggests lines on which research may profitably be based. Certainly the message as ultimately delivered to the cortex after passage through so complex a pathway must differ greatly from its original form. Had time allowed, it would have been tempting to prophesy what we may hope to learn from further investigation by modern methods about the sensorium itself. Our ignorance is for instance abysmal about the functions of the corpus callosum, that immense band of fibres nearly 200 million in number, which connects the two cerebral hemispheres. But that would have been pure speculation and I have preferred to restrict myself to more solid ground. The sensorium is after all the terminus, whereas the title of this address is the pathway leading to that terminus. The lower levels of the pathway still offer baffling problems to the experimental physiologist and it is certain that the clue to many of these problems lies in ideas derived from the study of reflex action.

THE EFFECTIVE USE OF MANPOWER

Address by

ALEC RODGER

PRESIDENT OF SECTION J

At the end of the last war we in Britain adopted a policy of full employment. The adoption took place—gradually, of course—in circumstances that involved some restriction of the size of our potential labour force. One important limiting condition was the raising of the statutory school-leaving age. A second was the introduction of compulsory national service. Together, these kept hundreds of thousands from the labour market. Other lesser influences soon began to operate, without direct legislation. Among them was a tendency for young people—grammar school boys included—to stay at school even beyond their expected time. Yet another was an enlargement of the flow from the schools to the universities.

All this happened at a time when we were being told, repeatedly and urgently, that we must increase our exports or perish; and we were trying to obey. Our resultant manpower difficulties were not lessened by the fact that concurrently our working population was beginning to feel the considerable impact of an earlier decline in the birth-rate.

It was not surprising that appreciation of manpower economy problems, already stimulated by war, sharpened. Devices old and new—from joint consultation to electronic computers, from management training schemes to fork-lift trucks—were hopefully advocated for their at least partial solution. And more careful attention than ever before was given to certain special groups of workers—in particular, the young, the ageing and the disabled—whose harnessed efforts, it was considered, might improve our chances of making a good recovery from recent misfortune, and help to pay for the welfare state we had planned for ourselves.

Though for economic, demographic and other reasons their shape is not what it was ten—or even five—years ago, problems of manpower economy continue to exercise our minds. They do so most vigorously and persistently in industries in which—because of the nature of the work, or its unpopularity—labour costs are high, as they are in coal-mining, where they account for 60 per cent. of the total costs (Sales, 1955). But—despite some planned overmanning and feather-bedding in a number of large concerns—there is no industry in which anyone in a position of responsibility, on either side, regards this matter as unimportant. Nor are our industries alone in their worries.

It is my intention to offer you an occupational psychologist's reflections on certain of these manpower problems, mostly in the form of proposals for action rather than as a record of achievement; and I shall begin by saying what I mean by the label I have given myself.

What Occupational Psychology Is

Occupational psychology, as I see it, is both a science and a technology: it encompasses not only the discovery and organisation of knowledge but also its well-contrived use. The knowledge the occupational psychologist is interested in is knowledge about the behaviour of people as workers of every kind and every level. As a scientist he seeks better understanding of the internal determinants of their behaviour; and as a technologist he helps, through that understanding, to improve their effectiveness at work. When he does all this in an industrial setting he is an industrial psychologist; and when he does it in a military setting he is a military psychologist.

It is of course as a technologist, an applier of scientific knowledge, that the

occupational psychologist is best known ; and in this role at least he can usefully describe his fields of interests in terms of the main problems he helps others to solve. These are, basically, the problem of fitting the man to the job (through the improvement of methods of vocational guidance, personnel selection, and occupational training) and the complementary problem of fitting the job to the man (through the improvement of methods and organisation of work, equipment design, and working conditions of diverse kinds) (Rodger, 1949).

They are problems he ' helps ' others to solve, because his technological function must be essentially advisory, not executive. Even as an adviser he may be, and often should be, one of several. He does not set himself up as *the* expert on any of the problems I have enumerated. His particular brief—and this is the foundation of my argument this morning—is to look at all of them with his mind on those internal determinants of human behaviour which I shall call, broadly, capacities and inclinations. That is, he is concerned everywhere, and all the time, with those personal attributes which govern what people, as workers, *can* do, and what they may be *inclined* to do. The whole point of his training as a psychologist has been to make him a specialist in this sphere ; to make him more precise than others in his knowledge of the nature, normal development, pathology, assessment and use of individual human capacities and inclinations.

Lest this claim sounds arrogant, let me admit freely that some people without psychological training cut a more impressive figure at some of these things than some people with such training. That does not invalidate my contention. It is possible that with psychological training many of these ' natural ' psychologists, these ' commonsense ' psychologists, would be very much better still.

In short, then, no matter what sort of problem it is he is helping others to solve—whether it is concerned with fitting the man to the job or with fitting the job to the man—the occupational psychologist's practical task is to bring to bear on it, in a skilful fashion, our accumulated knowledge of human capacities and inclinations. If he cannot do that better than the next man, there is no justification for his existence.

To this series of bald pronouncements, let me add a qualification. For some of you may well be asking, ' What about other related sciences—physiology, for example, on the one hand; and sociology on the other ? ' It is clear that the physiologist, as well as the psychologist, is concerned with human capacities and inclinations ; and that the sociologist, even though he is not concerned with such personal attributes directly, is at least concerned with some of the circumstances that mould them and are moulded by them. It is not my intention to embark here on an examination of the links between psychology, physiology and sociology ; but let me suggest in passing that the connection between psychology and physiology deserves more rigorous treatment than it receives in certain popular textbooks of psychology, where we are still fobbed off with unconvincing distinctions between muscles, glands, nerves and whatnot, to be found at one level, and something called ' the individual as a whole ' to be found at another —higher—level. I wonder whether it is not sometimes both convenient and salutary for the psychologist modestly to regard his science as what I have called elsewhere (1949) ' residual ' ; to look upon it, indeed, as a biological science dealing with ' left-overs ', whose function is to take up the explanation of human behaviour at the point at which the physiologist has to say, ' I'm sorry, but I can't do any more—not yet, anyway.' This is the point at which human capacities and inclinations become so hard to think about and detect in the physiologist's way, that other explanatory concepts and other methods of investigation are needed ; the point at which we cease to talk of a man's muscles, his glands and his nerves, and talk instead about his general intelligence and his interests, or about his opinions and his attitudes, or even—if we are so minded—about his ego and his id. The task of the psychologist, on this view, is primarily one of clearing up after the physiologist. And if I am accused of minimising the scope and importance of psychology in expressing it, I must reply that there appears to be plenty of useful clearing up to be done. Certainly, at the present time, what is left

over when the physiologists have done all the explaining they can manage is varied, complex and extensive enough to justify confidence that psychologists need have no fear of working themselves out of a job. But there is, too, the fact that the psychologist, in playing his part in the explanation of human behaviour, may help the physiologist to play his. As Malmo (1954) has remarked, ' Certain behavioural observations suggest, even demand, the presence of certain physiological mechanisms before they are discovered by the physiologist.'

Such, then, is the base from which I approach my theme, the effective use of manpower. In discussing it, I shall assume that it is desirable that manpower should be used effectively ; that is, on the view I am adopting, that what we call our ' work ' should make satisfactory use of our capacities and be in satisfactory accord with our inclinations. The word ' satisfactory ' or some equivalent is needed, because clearly we may experience very great difficulty in using all our capacities in our work, and we may have inclinations that are in conflict with one another. In the event, we may have to compromise : some of the components in our psychological armoury may have to be modified, and others may have to find expression outside our work. My assumption about the desirability of the effective use of manpower is one that I ask you to share with me, at least for the next forty minutes. There are, of course, various grounds on which you might do so, from the ' national economy ' plea to the ' enlightened self-interest ' argument. Some of you may be inclined to accept, as I do myself, a religious contention of the sort that seems implicit in the first item of the 300-year-old Westminster Catechism, which announces, firmly and uncompromisingly, that ' the chief and highest end of man is to glorify God and fully to enjoy him for ever.'

The Unity of Occupational Psychology

I shall marshal my reflections under six headings. The first three—vocational guidance, personnel selection, occupational training—belong, you will recall, to the group of problems I have labelled ' fitting the man to the job '. The other three—methods and organisation of work, equipment design, working conditions—are problems of ' fitting the job to the man '.

But let me introduce here a general point I shall emphasise later. These six fields of interest of the occupational psychologist are contiguous and not easily separated. What seems to be a personnel selection problem may turn out to be largely a training problem. Many organisations have hankered after better personnel selection procedures to enable them to improve the quality of their operatives, of their apprentices, of their clerks, of their salesmen, of their supervisors, when what they have needed in the first instance has been a better training method to enable them to make more effective use of the people they already have available. Similarly, a methods of work problem may be revealed as—at least primarily—a working conditions problem. However substantially a new method may be capable of increasing productivity, it may in fact prove highly disappointing, if those who are asked to use it pursue restrictive practices, either of the well-recognised kind derived from group pressures or of the neglected but important variety which arises quite simply from private quarrels between fellow-workers. A major cause of slow development in occupational psychology has been a continuing disregard of the unity of our problems, a disregard of which Hearnshaw (1942) complained long ago, and which undoubtedly springs to some extent from the unfortunate fact that those who are interested in fitting the man to the job tend to lack enthusiasm for fitting the job to the man, and vice versa. This we must recognise as a serious matter, because it may lead to the dissipation of precious research funds on projects too narrowly conceived. An investigator whose concern is mainly with fitting the job to the man may be reluctant even to explore the possible influence, on work performance, of persisting individual differences in capacities—for instance, in what some call general intelligence—which are often to be found to be important by those whose concern is mainly with fitting the man to the job. He may indeed be inclined to assume that for his purpose all

men may be regarded as equal in their capacities, or nearly so. If he makes this assumption, it is natural that he should not worry greatly about the nature or size of his population samples. At a recent conference at Cambridge, at which several papers were read by psychologists, the only contribution that offered clear recognition of the importance of this sampling problem was presented by an anatomist ; and he came from Oxford.

Vocational Guidance

Now let me tackle the first of my six specific topics, vocational guidance. There is, surely, a case for supposing that arrangements for the giving of wise occupational advice to individuals would be generally regarded as the firmest foundation for any plan for the effective use of manpower. We have learned (Kristy, 1952 ; Connolly, 1955) to be cautious about making the assumption that satisfaction with work will be closely associated with satisfactoriness in it ; but it seems probable (see Heron, 1954) that people who are not suited to their work by both capacity and inclination are unlikely to become highly productive. Yet in dealing with this matter we in Britain are well behind some of our main trade competitors. And it is difficult to escape the conclusion that our backwardness stems from a mixture of unfavourable attitudes about which we know far too little. It is occasionally suggested that vocational guidance is unimportant, because people—especially young people—are very adaptable ; or that it is useless, because even in days of full employment most people have to take what they can get locally ; or that it is undesirable, either because it is good for people—again, especially young people—to knock around a bit or because we should not interfere with people's lives. Of course, there is something worth discussing in all these views, and in several others commonly expressed : so there is in the attitude of many employers, who are not unnaturally anxious lest potential employees providentially situated on their doorsteps should be steered elsewhere. Nevertheless, it seems unquestionable that here, in the vocational guidance field, we have a serious gap in our defences against waste.

In saying this, I do not overlook the work of the Youth Employment Service. If we take account of the conditions under which it has to operate—including, as Dobinson (1955) has pointed out—the inadequate pay and therefore status of its officers—we must agree that this Service manages very well. However, if we look at it in the light of the far-sighted report made by the Ince Committee in 1945, we can readily see that, despite advances in recent years in such matters as the training of its officers and potential officers, it is not yet doing all it should or could. It offers occupational information ; it gives advice ; and, after placement, it conducts what are called reviews of progress. But, mainly because of the unfavourable attitudes I have mentioned, it does all these things with a very delicate touch indeed. The caution with which the Service is forced to tread is plainly illustrated by the restrictions placed on teachers in preparing the reports they make to youth employment officers on their school-leavers. While they are asked to provide information on a boy's health and physique, his educational attainments, his general intelligence and his specialised aptitudes, they are expected to keep quiet about their knowledge of his interests, his qualities of disposition and his circumstances. It is out of order for them to write, for example, that John Jones is a hard-working boy, and that he has for a long time been keen on mechanical things and eager to follow his father into a skilled engineering trade. This, we must realise, was the doing not of the administrators of the Youth Employment Service but of our legislators, who apparently believe it is inexpedient to allow youth employment officers all the help they could obtain from very knowledgable people. It is perhaps fortunate that, in practice, some teachers display only limited familiarity with the rules governing their report-writing.

From the occupational psychologist's standpoint, the chief and most pressing need of the Youth Employment Service is for systematic, long-term 'follow-up' studies designed to reveal, by the independent application of appropriate criteria, the strong and weak features of currently-used concepts and methods. Inevitably, in a period of labour scarcity, this idea causes apprehension among

employers, who fear that such studies would have an undesirably unsettling effect on the samples of young workers from whom, and about whom, enquiries would be made. But, as the Ince Committee recognised, we cannot hope to plan methodically and well for the future without facts. On the availability of relevant, accurate, comprehensive follow-up information depends the solution of all our major problems in this field, including the most fundamental of all, that of deciding how to group and grade occupations for vocational guidance purposes. It is plain that what is required here is a substantial series of integrated studies of the main things, psychological and sociological particularly, which, by a method of contrasts, can be shown to differentiate those who succeed in an occupation from those who prove less successful in it or who fail (Rodger, 1939, 1945).

In another part of the vocational guidance field—that covered by the Ministry of Labour's Industrial Rehabilitation Service—the outlook is brighter, mainly because this Service has been able to get on with its duties unobtrusively and in its own way. It has built up for itself a good international reputation ; it has developed a research outlook ; and (not the least important point) it has acquired the moral support of the Select Committee on National Expenditure. In fifteen industrial rehabilitation units (IRU's) in Britain, men and women of all ages who have suffered injury, ill-health or long unemployment are taken for courses lasting an average of two months. They are 'toned-up', as the saying goes, in workshops, gardens and gymnasia. They are not given occupational training, for that is not the object of the Service. But as about 8,000 of the 10,000 dealt with by the IRU's in a year are unable to return to their old jobs, fresh employment or training recommendations are made for them on the basis of observations compiled systematically by a team which includes a vocational officer who is usually a psychologist. The atmosphere of an IRU is that of an industrial establishment, not that of a hospital or convalescent home where everyone is a 'patient'. A methodical follow-up scheme of a simple but useful sort is in operation ; and a unit with special research and development functions was opened last year.

There can be little doubt that, in this area of industrial rehabilitation, the Ministry of Labour is providing the occupational psychologist with an opportunity of proving his worth which is to some extent comparable with the opportunity given to him by the Service Departments during the war. That does not mean that he has no exasperating frustrations : what it does mean is that he has a good chance to advance in relevant knowledge and skill ; and to show that he can do a practical job of work that satisfies three criteria put forward on another occasion (Rodger, 1948). In my view, it is of the utmost importance that all psychologists in public services should exert themselves to make their work technically sound, administratively convenient, and politically defensible.

Let us hope that the Ministry of Labour will soon be able to regard their IRU's not merely as places intended to take people for courses lasting several weeks, but also as centres which will, on the basis of only two or three days' attendance, provide help for the Ministry's employment exchanges in the tackling of more ordinary vocational guidance problems. A precedent exists in an arrangement whereby some units give assistance to youth employment officers in difficult cases. Eventually, perhaps, we shall see vocational guidance bureaux for adults, basing their activities on the experience gained in IRU's and elsewhere, attached to all major employment exchanges of the Ministry. This is desirable, for there could be no more convenient way of ensuring that well-considered advice is available to the community as a whole. From their very nature, the Youth Employment Service and the Industrial Rehabilitation Service are concerned with special groups ; and in times of change that may involve many mature and healthy people in substantial modification of the character of their employment, we cannot possibly rest content with our existing facilities.

There are other adult vocational guidance problems awaiting our attention. One, in particular, I must mention ; because if we are justified in assuming that a person is unlikely to be maximally

productive unless he is satisfied with his work, it is important. It is created by the setting-up in large organisations, public and private, of what are designated ' general ' classes of employees. The situation is typically one in which a number of previously fairly specialised small occupational groups—engaged, for example, on this and that kind of clerical work—are absorbed into one big group—for example, a general clerical class—with the laudable intention of providing both more flexibility within the organisation and more adequate opportunities for promotion. The ' generalist ' system tends of course to involve a person in more changes of work than the ' specialist ' system. Not infrequently he finds that, when he wants to move onward and upward, he is expected to ' broaden his experience ' by taking assignments of little direct interest to him. Here we have a source of waste that unquestionably needs investigation. The authorities would be wise to take appropriate chances of comparing the satisfaction and satisfactoriness of specialists and generalists engaged on the same type of work in different organisations ; and of specialists in an organisation before and after their absorption into a general class in the same organisation. It may be that some of the more obvious advantages of the general class notion are cancelled out by its tendency to produce a higher proportion of people who have no enthusiasm at all for some of the jobs they have to do. And it may therefore be that, if general classes are to be worth having, those who arrange their work and allocate their members will have to take more account of what should be essentially a vocational guidance approach to their problems. I do not suggest that organisations which have general classes have not considered this matter at all ; but I strongly suspect that thorough, open-minded study of it would prove to be rewarding, not least in the Civil Service.

Personnel Selection

After vocational guidance comes personnel selection. Here we are thinking of the man or woman, the boy or girl, who—we may suppose—has been given advice on the choice of an occupation and is now a candidate for a particular appointment or for a particular training course. It is often said in these times of full employment, ' Our selection procedure just doesn't work nowadays : we have to take anybody we can get.' The comment suggests that many have been misled by a name. For a personnel selection procedure is primarily a procedure for the assessment of capacities and inclinations ; and if it is appropriately planned it is usable not only for selecting and rejecting but for other purposes too. I would argue —as indeed I did a moment ago, by implication—that in our present circumstances there is a considerable need for careful stage-by-stage assessment procedures designed to enable employers and training institutions to make the most effective use of their recruits, by providing a suitable service extended in time and midway between guidance and selection. Might not that three per cent. annual increase in productivity, which we are told we need, be achieved by this means alone, perhaps ? It is true that the notion operates already to some extent, for example, in the ' executive development ' schemes of some big firms ; but there is not much sign yet in industry of the systematic and continuous study of the capacities and inclinations of people below the management level. However, the mere fact that it is appearing high up is presumably a sign that the boardroom thinks it may be on to something good, so perhaps we may be cautiously optimistic. We must, of course, be wise in our formulation of assessment procedures. A rashly contrived scheme is very likely to defeat its own ends by frightening potential recruits away and antagonising those who remain.

Let me remark, in passing, that in advocating what would be essentially a running audit of the capacities and inclinations of a whole organisation, I am not pleading for a nightmare of psychological testing. The corner-stone of the personnel audit, like the corner-stone of ordinary vocational guidance, should be well-designed and conveniently-operated reporting procedures, which aim at the quick and painless extraction of thoroughly relevant facts and opinions from knowledgable people. On this method of preparing to make effective use of human capacities and inclinations, industry has

much to learn from the Civil Service (Menzies and Anstey, 1951) and from the Fighting Services. For reporting on people below management level there is, in my view, great potential value in the type of workshop report form used in IRU's, where a supervisor is asked to rate his workers on a short series of five-point scales, composed of items defined at the extremes of the scales by opposed descriptive statements expressed in ordinary workshop language (Cavanagh, 1950).

On management selection specifically I shall venture only one comment. It is this. It seems high time some responsible organisation embarked on the delicate but fundamental task of studying management 'requirements' by the method of contrasts I have mentioned in my remarks on vocational guidance. Its aim would be to distinguish between the capacities and inclinations of managers who by agreed criteria are held to be successful and managers who by the same criteria are held to be less successful (Rodger, 1951). It might, of course, be found that the word 'management' covered duties so many and various that useful generalisation would prove very hard indeed. It might be revealed also that accepted standards of performance in them differed so much, from place to place and time to time, that they served merely to drive home the fact that what we call the requirements of any occupation are always dependent on the state of its labour market. But even negative findings of these kinds would be valuable if they helped to protect us from the unverified—and often unverifiable—statements about management selection and training which flow rather freely in some business circles.

Occupational Training

Now I turn to my third heading, occupational training. Clearly, a good programme for the effective use of manpower must ensure that sound guidance and sound selection or allocation are followed by sound training: all are important phases in the process of fitting the man to the job. There are, to my mind, three parts of this field which should increasingly engage the attention of the occupational psychologist in industry—those concerned with the training of managers, apprentices, and operatives.

In the management field there is a particular need to explore ways in which men —and maybe women too—with an arts background can be turned into production managers. War-time experience in the selection and training of potential engineer officers suggests that we have no right whatever to assume that a young man who has followed a predominantly arts course to the age of eighteen, or even later, cannot hope to become competent in an occupation which demands quick appreciation of things technical. As some firms have already found, a useful proportion of arts people have capacities and inclinations which would enable them to function quite differently without serious difficulty, given suitable training and experience. Nor need the discovery of these occupational hermaphrodites be a laborious and hazardous affair of trial and error, for there is probably no problem the occupational psychologist has tackled more successfully than that of helping an organisation, civil or military, quickly to detect technical potentialities in apparently unlikely people.

When we look at apprentice training, we should be greatly disturbed by what we see. A prominent administrator in the field of education has recently described the apprentice system as 'archaic'. It is more than that: in its commonest forms, it is wasteful too. Fortunately, we are gradually realising the fact, helped rather than hindered by some of the system's most determined supporters, who lend their aid by their refusal to consider changes in their requirements in regard to age of entry and length of apprenticeship. For, as I have remarked, many boys remain at school considerably beyond the upper age limit for acceptance for most apprenticeships, and are therefore precluded from entering them. And, of course, these tend on the whole to be the abler boys. They are being lost to trades which need them. In the meantime, some employers' associations and trade unions who have seen the red light are stealing a march on the reactionaries. For example, in what is appropriately called the galvanised section of the wrought hollow-ware trade, a five-year apprenticeship has been turned into a three-year training course, which a boy may start at

any time between the ages of 15 and 17 (*Ministry of Labour Gazette*, May, 1955). This seems very much wiser, under present conditions, than the sort of arrangement whereby the period of apprenticeship is a five-year affair and must start not later than 16. There are, no doubt, some arguable economic reasons why apprenticeships should be completed by the age of 21, but surely it would not be impossible for this idea to be retained, if it must be, and yet for more flexiblity to be introduced into the rules about starting-ages and length of training? Certainly, it would seem expedient to allow apprentices some credit for having continued their ordinary education, provided they have reached suitable standards in it.

But this would provide only a partial solution of the problem. We need a thorough examination of the system and all the good and bad restrictive practices associated with it. Let us face the fact that many apprentices are not being trained in a profitable way; that these young men often waste their time on jobs no one would dare to mention in a printed syllabus; that they are often kept on particular jobs far too long, because production is given priority over training; and that they often stand no chance of gaining the breadth of experience the potential journeyman should have. It is true that in some industries all these problems are receiving attention. Requirements are being modified; training is becoming more systematic; and exceptions to rules are being made. But we must not imagine that progressive-looking agreements reached on these matters at the national level are always acted on at the local level: they are not.

The problem is important, because the coming of automation may soon force us to review all our ideas about training, all our ideas of who should be trained for what and how. And it is urgent, because a minor manpower crisis already looms ahead. The Government Actuary has calculated that in 1962, only seven years from now, the number of young people attaining the age of fifteen, the present statutory school-leaving age, will be roughly 50 per cent. above the 1955 figure—927,000 instead of 625,000. How ready is industry to take advantage of the fact that for every two boys of high capacity leaving school today there will be three in 1962? Now, clearly, is the time for fresh thinking on the training of young people, especially for the skilled trades; and for reflection on relevant evidence of the sort already assembled in military and civil establishments which have managed to produce skilled adult workers for various trades in far less time than is commonly admitted to be practicable. The medical profession have long since dropped their apprenticeship approach to training, but—particularly, in recent years, through their further development of the system of junior hospital appointments and trainee assistantships—have conserved its most valuable feature, by ultimate attachment of the newly qualified doctor to a 'master' who provides experience in the practical use of the technologies of medicine and surgery (White Franklin, 1955). Why should not this be done more widely in the skilled trades?

The training of people to operate machines of one kind and another in industry and the Fighting Services has begun to present the occupational psychologist with some knotty problems, and research in this field is gradually eliciting new facts, especially about the so-called 'perceptual' element in operating skills, which may prove important. I say 'may', because much of this work is conducted in a laboratory setting, on a restricted range of operations, and with small groups of subjects who lack the ordinary industrial trainee's inducements to learn and are often of much higher intellectual calibre. Generalisation from facts revealed in such circumstances must be hazardous. Moreover, it may turn out that some of these enquiries are appreciably less relevant to industrial than to military operations. For example, studies of some of the stresses which corrode skills may continue to be of practical importance to the Fighting Services when the development of automation in industry has reduced their value elsewhere. But certainly the need for other investigations in this group—for instance, on the effects of stresses associated with increasing age—must be regarded as pressing. The time is not far off when one-fifth of the population of the country will be of pensionable

age, by present standards; and presumably another fifth will have to be regarded as 'getting on'. One useful finding of research on ageing (Welford, 1951) has been that what is often needed for the older worker is not 'light' work but less strenuously paced work. And if we put together the main conclusions emerging, we should probably decide that a substantial contribution to the effective use of manpower could perhaps be made by the re-training of many machine operators in their forties, for work of other kinds, while they are still trainable.

Methods and Organisation of Work

From my first trio of topics (guidance, selection, training), grouped together as problems of fitting the man to the job, I now pass to my second trio (methods and organisation of work, equipment design, working conditions) grouped together as problems of fitting the job to the man.

The investigation of methods and organisation of work in industry, known under various titles of the 'motion study' and 'methods study' sort, has of course been hampered by fears of rate-cutting, overwork and redundancy, as well as by sheer dislike of change. The fears have not been without foundation, and for this reason such problems as these cannot in practice be divorced from problems of pay, security and status, which belong to the last of my six topics, working conditions. But let us for a moment try to consider them in isolation, to identify the nature of the occupational psychologist's concern with them. Is he merely another efficiency-monger to be entered on the list as a straight competitor of the industrial consultant, the work-study practitioner, the time and motion study engineer, the production engineer, the methods engineer, the costs and works accountant, the organisation and methods man, and all the rest? From my early general remarks about his function, you will readily predict that my answer to these questions is, No, he is—or should be—once more a collaborator. In day-to-day studies of methods of work, his particular business is to help to ensure that new procedures are suited to the capacities and inclinations of those who will be expected to use them. If he is competent, he will be able to perform this task with greater precision, speed and economy than others.

But he has important research interests in this field. Let me mention three of them. In the first place, though extensive use is made in methods study of certain principles of motion economy enunciated by Gilbreth (1923), (Barnes (1949) and others, we know very little indeed about the circumstances in which they do and do not hold good. Secondly, the time studies which are widely used as a means of comparing the relative values of different methods, as well as—more extensively—for rate-setting, usually have no scientific leg to stand on because of the nature and extent of the arbitrary judgments they involve (Winston Rodgers and Hammersley, 1954). What is going on in a time-study man's mind when he 'rates' a job as a 60, an 80, or what-have-you? How exactly does he make his subsequent 'allowances' for what is called 'compensating rest'? How does he, especially when he is in a hurry, decide which workers he is going to study? We know hardly anything about the answers to these questions; and until we have them in reasonably satisfactory form we shall be entitled to wonder whether a good deal of current practice in time study is not perhaps a piece of mumbo-jumbo, developed—by agreement between managements and workers—to provide an apparently rational basis for the payment of wages whose amount has already been settled by quite other means. It was the research director of the Textile Workers' Union of America (Barkin, 1954) who recently said: 'Time study . . . is a procedure for careful personal evaluation of a job and human application in terms of vague, ill-defined personal norms held by the analyst.' A third problem deserving of attention here is that of discovering more about the actual functions, status and qualifications of practitioners in this whole field, now generally known as the field of work-study; and about the ways in which they are related—informally as well as formally—to other technical men in their organisations, to management at various levels, and to workers and their representatives.

Equipment Design

My fifth heading is equipment design. The occupational psychologist's concern with this is well indicated by a notice which Sir Philip Clarke, lately Director of the Naval Electrical Department, spotted on a notice-board in the Pentagon at Washington. It ran: 'No research or development project or production item is satisfactory until it is successfully in use by the user. This means in use by the personnel who are there, not by the personnel we would like to have there. The material must be designed for that user.' The point is clear and need not be laboured. Considerable progress has been made in this field, much of it through the work of the Medical Research Council's Applied Psychology Unit at Cambridge, which has supplemented in a valuable way the investigations of physiological and other scientists and technologists. But quite recently Sir Frederic Bartlett (1955), the former director of the unit, had to deplore the fact that even in weapon development the physiological and psychological tolerances of the human operator were still frequently ignored. He rightly warned those who put their faith in human adaptability that this adaptability had limits, and that stresses of various kinds, likely to be created in operators should all be taken properly into account at the design stage.

But it is not enough that the capacities and inclinations of operators of equipment, military or industrial, should be considered. Attention must be paid to the maintainers too. The importance of this was well brought out by Dummer and others (1955) at a meeting of the British Institution of Radio Engineers, held under Admiral Clarke's chairmanship to discuss the maintainability of equipment in the Fighting Services. As I listened, my thoughts went back to the war period, when I was in charge of the Admiralty psychological department, and particularly to an unfruitful minute I once wrote on ways of coping with an alarming dearth of suitable candidates for training for the radio mechanic branch of the Navy. For here were the same points being made again—on the urgent need for the simplification and standardisation of equipment, for the devising of clearer fault-finding procedures, for the provision of fast and slow courses of instruction, and for the removal of unnecessary theory from the syllabus. The object of the minute was, of course, to argue that the radio mechanic branch could 'make do' with a smaller proportion of really able men, if co-ordinated steps were taken to fit the men to the job and fit the job to the men. The plan advocated would have had the advantage of leaving more of the outstanding people available for other branches of the Service, and for commissions. The recent BIRE meeting made it obvious that the desirability of approaches of this kind to the manpower distribution problem—approaches which take account of quality as well as quantity—is now widely accepted, at least in the Service Departments.

However, we cannot afford to be complacent about the matter. We are told that automation is upon us; and though we may suspect that its effects on the working population of Britain will be softened by our arrangements for full employment, and by slow development resulting from the fact that our industry is not only rather conservative, but is also made up largely of small firms with little capital, we must begin to think seriously about the manpower problems it will produce. Some of them were discussed in a general way a few weeks ago at a conference of the Institution of Production Engineers, but no one seems to have had much to say about certain serious implications of the change that automation tends to produce in the proportion of skilled to unskilled workers. According to Lilley (1955), the Moscow factory which makes pistons for the whole of the Russian light car industry is worked by a staff of nine men on each shift: one controller at the electronic nerve-centre, one labourer, two machine-minders (who appear to be at least semi-skilled), and five skilled maintenance men. That is, two-thirds of the team are of the skilled trades level. If this is the shape of things to come, how are we going to manage? It is perhaps a bit fanciful to suggest that we may create a situation in which the abler half of the working population have to work themselves silly to earn enough to pay enough taxes to keep the other half sitting around doing nothing; but reflection along these

lines would probably do us no harm. It might at least help our designers and their employers to recognise that transfer machines, automatic assembly equipments and such like must be matched to the capacities—and of course to the inclinations—of those who are actually available to operate and maintain them. It might also help us all to see the importance of encouraging young people to exploit their capacities and inclinations fully. Some months ago the Glasgow Productivity Committee (1954) reported that, of the hundreds who enrol for first-year trade courses in the Glasgow area, 10 per cent. never attend a single class, another 20 per cent. cease attendance before Christmas, another 10 per cent. do not return in the New Year, and only 40 per cent. take the first-year examinations. Only 25 per cent. of the original enrolment begin a second year, and only 12½ per cent. persist to a subsequent year. Eventually, 8 per cent. enter for the trade examination, and only 5 per cent. obtain some form of qualification. The wastage in Glasgow may not be typical of the country as a whole, but it exemplifies what is undoubtedly a big problem in many places. Should we not take pains to supplement Glasgow's own useful attempt to discover how the trouble arises, and how it can be cured? Here is another item for our list of research agenda, which clearly demands collaboration between employers, trade unions and technical institutions on the one hand, and economists, psychologists and sociologists on the other. Of course, we cannot expect to deal with the problem once and for all, because presumably the influences at work are of a constantly-changing kind.

Working Conditions

Now I come to the sixth and last of my special topics, working conditions. In recent years the occupational psychologist has devoted less attention than formerly to the effects of working conditions of the physical environmental kind (heating, lighting, ventilation, and so on) and more to the effects of those which—perhaps because they influence the worker's inclinations rather than his capacities—tend nowadays to be gathered up under the rather flabby title 'human relations'. Here he is, in fact, touching on some of the stark and explosive realities of management organisation, payment systems and restrictive practices.

In the study of physical environmental conditions he has accomplished a great deal, particularly, in this country, through the Industrial Health Research Board and other Medical Research Council groups. But his progress in the rest of the field has been slow. It is true—as Viteles (1954) has shown recently—that he has, in collaboration with others, accumulated a considerable mass of data. But a remark made a quarter of a century ago by Murphy and Newcomb (1931) about social psychology seems applicable here: 'Research has been spotty, scattered, uncoordinated.' It is time we asked why.

I have three explanations to offer, to supplement others occasionally put forward which range all the way from the conservatism of industrialists to a shortage of psychologists. In this brief additional list I seek to blame no one but ourselves. In the first place, the occupational psychologist has too often been unclear about his own specific role, and has in consequence been inclined to dabble into other people's areas to the neglect of his own. This tendency has increased, it seems to me, with the growing popularity of the multi-disciplinary approach, whereby mixed teams of investigators tackle a human relations problem. The idea is, of course, admirable; but the organisers of such teams appear sometimes to overlook the fact that a team should be composed of experts, each with his own particular function to perform. We should not expect much of a football team composed of people of the 'can play in any position' variety. Nor can we expect much of a research team so composed. Secondly, the occupational psychologist has frequently sought after wide generalisations when he should have been satisfied with something narrower. If more often he asked himself, *In what circumstances* is X true? instead of simply, *Is* X true? he would more rapidly build up a useful body of knowledge. The formulation of his problems in this way would compel him to be more comprehensive and thorough in his initial survey of the conditions likely to affect his findings. Moreover, it might incidentally make him less

disdainful of the use of certain statistical techniques—for example, of factor analysis—which might improve his classification of the variables he decided to take into account. Thirdly, he has been too content to do his job as an outsider, as a visitor. It is unquestionable that some of the best enquiries in the human relations field have been conducted by people who, through well-cultivated informal acquaintance or formally, have ' belonged ' to the organisations whose problems they have studied. No doubt there is a place for the peripatetic research worker who has his base in a university department or an independent research body, and who comes to his task with a fresh eye and experience of comparable problems elsewhere ; but it may be a smaller place than we academics like to suppose.

It seems to me that until we attend to these things—that is, until we clarify our roles, limit our objectives, and work more from the inside—we are not likely to make rapid advance, because few will feel that we are likely to prove competent in tackling crucial problems. We shall be left on the periphery, having little direct contact with the people who make far-reaching decisions and little knowledge of their real worries.

Let us consider, very briefly, an illustration. We are all aware that the coming of our welfare state has thrown up several big problems in this field of working conditions, including problems created by the narrowing of wage-differentials. For instance, as Roberts (1955) has pointed out, in the building industry we find that, after a period of four hundred years in which unskilled men received two-thirds of the pay of skilled men, they now receive nine-tenths. Why should it be left to sociologists, like Caplow (1954), and economists, like Barbara Wootton to reflect on the psychological issues this wage problem raises ? We may be glad they have tried their hand at it, for they have both been provocative and Barbara Wootton has been entertaining too. But should not the occupational psychologist be concerned with the matter himself ? Gaitskell (1955), in a recent interview, said that his particular political party wanted to see wage-differentials based more securely on occupational differences in skill, responsibility, effort, danger and dirt. Surely, in the study of all these criteria—and others that might be added—the psychologist has a very substantial part to play ? Are they not all matters of human capacity and inclination ?

But is he likely to have a chance to play this part, if he does not make his function clear, if he does not formulate his questions well, or if he stays on industry's touchline ? Does it not seem obvious that, if he is to be concerned with crucial issues, he must more often be *in* industry ? I should like to see more psychologists within industrial and commercial organisations, spending part of their time on useful bread-and-butter work, paying their way in, for example, the fields of selection, training and work-study ; and part of it in cautious development and research in their other fields of interest. This is, in fact, the pattern of psychological activity we find already in certain Government departments and industrial concerns. In my view, we should seek to extend it, for it may offer us our best approach to some of the tricky working conditions problems we should be helping our users to solve.

Conclusion

In conclusion, let me say in a few sentences what I have tried to do in this address, and what I have tried not to do. My first object has been to map out the occupational psychologist's fields of interest, using for this purpose the language of those who enlist his help ; and to suggest that it is convenient to group these fields of interest under two complementary headings—fitting the man to the job, and fitting the job to the man. My second object has been to illustrate his concerns within these fields, not so much by reference to past achievements as by comment on problems now arising in the world of work. My third object has been to press the point that in all his fields, and all the time, the occupational psychologist is concentrating his attention on the study of what I have called human capacities and inclinations : for this is his peculiar contribution to the solution of scientific and technological problems. That, in short, is what I have tried to do. What I have tried not to do is to make out that the occupational psychologist is *the* expert

in tackling any of them. Without others —the physiologist, the economist, the sociologist, especially—he is not likely to get very far. But, as I have said, in his collaboration he must constantly remind himself of the nature of his specific function. No science, no technology can hope to thrive unless whose who follow it are clear—quite clear—about their own particular duties and responsibilities.

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BIOLOGICAL ORGANISATION

Address by

PROF. K. MATHER, F.R.S.

PRESIDENT OF SECTION K

EVERY branch of science is concerned with organised systems ; with the nature and relationships of the parts from which the systems are constituted, the means by which they are built up and broken down, and their properties in relation to those of their parts. The physicist meets these problems, for example, in his studies of the structure of atoms and atomic nuclei, and the chemist in his molecules and macromolecules ; but nowhere else is the range of levels and complexity of interlocking systems seen as it is in biology. The biologist's interest must stretch from the smallest entities which may be regarded as living—the genes and the virus particles—to those large aggregates which we call populations and communities, including human society. Nor is this range a unity only in name, to be broken down whenever we wish for ease of detailed consideration. On the contrary, as genetics is increasingly impressing on us, all the levels of organisation are interlocked and interdependent, so that a true understanding of one is impossible in isolation from the rest.

Genetics started with the modest—or at least limited—aim of understanding the relation between parent and offspring, but the pursuit of this understanding has taken it into every field of biological study, drawing from them all and adding to them all. It very quickly ran into the problems of nuclear organisation ; for even if chromosomes had never been seen, genetical linkage would have required them to be invented. The linear organisation of the genes into chromosome groups, and the phenomena of crossing-over and structural changes can all be inferred, and indeed have all been explored, from purely genetical data ; but the cytological observation of chromosomes has made its contribution to the story, too, by showing us the centromeres, specialised to govern the movements of the chromosomes, and the nucleolar organisers which mediate at least part of the nucleic acid cycle. Thus by a combination of the two techniques we see the nucleus as a system organised for the perpetuation and recombination of the hereditary materials ; a system with specialised structures and specialised organs, in which the various parts co-operate to produce an orderliness of cell inheritance achievable in no other way. And we have also been coming to learn that certain of the genes even have the further specialised function of governing the nuclear mechanism itself—the structure, movements and recombination of the chromosomes—so that the genotype governs its own means of propagation.

Thus we see that not only the action but the very mechanism of heredity depends on the permanence of the genes. But we have also learned that genes are not unchanging : occasionally they alter by a process which we have barely begun to understand and which we call mutation. The study of these mutations, induced by ionising radiations and by various chemical means as well as arising as we say spontaneously, is enabling us to go deeper into the genetic materials and to discover something of gene structure. The gene is revealing itself as a unit of action, whose characteristic mediation of a complex biosynthetic process depends on its parts being organised in a special spatial pattern. The principles of this organisation and the way in which action is related to physicochemical structure have yet to be worked out, but we are probing into a new level of organisation and one which is coming very close to the chemical. At this level, too, the investigation of virus structure, in

which genetics is also playing its part, has much to contribute, as has the chemistry of the nucleic acids upon which the reproduction of genes, viruses and similar entities clearly depends.

It is conventional to lump together all the living material of the cell outside the nucleus under the heading of cytoplasm. In doing so, however, we should not obscure from ourselves the great variety of materials, processes and even systems which the cytoplasm comprises. The organisation of the cytoplasm is, indeed, more varied, besides being less well understood, than that of the nucleus. Parts of it, such as chloroplasts, or the kappa particles of *Paramecium*, have all the permanence of genes, and while depending on nuclear genes for their precise properties of action and reproduction do so no more than genes depend on one another—or would if they were divorced from the rigid mechanics of the nucleus. Other cytoplasmic entities, notably certain of them connected with enzyme production in yeasts, arise initially as products of the nucleus but can maintain themselves thereafter in the absence of the initiating gene, provided the environment is suitable. Still other constituents of the cytoplasm will be even more fugitive, or more restricted in the genetic and environmental circumstances under which they can maintain themselves.

All these various entities play their special parts in the working of the cytoplasm, each dependent on the others, but little can yet be said as to how they are organised to keep in step with one another and to do their work. The mitochrondria may well play a key part in the organisation, so providing it with a constant mechanical basis in the way that the chromosomes provide a framework for the nuclear genes. The mechanical organisation of the cytoplasm is, however, manifestly less precise than that of the nucleus, so that balanced action will depend on chemically achieved equilibration more than on mechanical control. The nucleus provides the firm foundation on which this cytoplasmic balance can be built, but we must not forget that the cytoplasm has in its turn a reaction back on the reproduction and action of the nucleus, even if not on its fundamental genic content. This is seen especially clearly in fungal heterokaryons where the genetically different nuclei enclosed in a common cytoplasm may reproduce at different rates determined by that cytoplasm. The relative frequencies of the different kinds of nucleus can thus be adjusted to the requirements of the cytoplasm and through it to those of the external environment, so providing a genetical flexibility admirably suited to the opportunistic needs of a saprophyte.

Nucleus and cytoplasm are complementary and, therefore, inseparable in cellular organisation, the one securing the constancy needed for control and the other the flexibility required for adjusted action. This adjustment may be related to the environment as in the fungi; but it is shown also in relation to other cells at the next level of organisation as it is seen in the differentiated somata of higher plants and animals. These somatic systems of cells and tissues are the most familiar and, from the point of view of their physiological working, best known type of organisation, for, indeed, they have long provided one of the chief subjects of biological investigation. The division of functional labour, the means by which it is brought about during development, and the ways in which it is regulated during operation are complex, but wherever investigation has been adequate these have all been shown to be under genetic control. It is worthy of emphasis, too, that somatic differentiation requires not only that differences arise but further that they arise as precisely adjusted differences : the cells must follow particular channels of development, intermediate types being useless. The genetic control must secure, therefore, not only that certain differences are built up but that given these differences all other deviations are suppressed or at least kept down to a tolerable maximum : beyond the required limits cellular lability must be superseded by stability.

Somatic differentiation must spring from controlled changes within the cell and since successful differentiation depends in all its aspects on an adequate genotype, these changes must ultimately be under genic control. Yet the nucleus does not change characteristically in differentiation. Rather a constant nucleus must be stimulating and managing a changing cytoplasm,

which although it may react back on to the nucleus and in particular on to its later action, must be the subordinate. The genetical problem of differentiation, and, indeed, the problem of relating the two levels of organisation, cellular and somatic, is that of discovering how constancy in one organ secures and controls change in another.

In the multicellular plants and animals every cell is at once a complex organisation in itself and a specialized part of that organisation which we call the individual. In the same way individuals form parts of the aggregates which we call populations, societies and associations. Unlike the cells of a soma, however, the member individuals of a population have no organic connection with each other. Even so, their inter-relations may be such that we must regard the population as something more than a mere numerical aggregate of individuals, as, in fact, an organisation at a new level with its parts bound together in a less tangible but none the less real way.

To take a simple example, each individual plant of *Melandrium dioicum* has roots, stem and leaves with all the apparatus for establishing and maintaining itself. It is a complete unit in these respects. It also has flowers, but it is obviously not a unit in reproduction, for if it bears pollen it lacks eggs and *vice versa*. Two plants, one male and one female, are the minimum for successful reproduction. In respect of this function, therefore, the individual is not complete : it is but a part of a higher organisation to which both sexes must contribute. This reproductive bond exists widely among animals, so many species of which are dioecious, as the botanist would term it. It is less common among plants but we can find similar bonds widely expressed in other ways, notably in heterostyly and various forms of incompatibility ; for in these cases, though carrying both pollen and eggs, the one cannot fertilise the other within the single plant. As with dioecious species, at least two individuals are necessary for successful reproduction.

This example brings home to us a fundamental point. What is a complete unit in one respect may be but part of an organisation in another. Organisation confers functional efficiency by the division of labour among specialist parts. It arises and is maintained because of this efficiency in function. It cannot be discussed, nor the complete individual unit defined, away from the function which is being discharged. And in so far as one organism may have several functions, it may be complete in respect of one while being part of a greater organisation in respect of another. This is, of course, true at every level of organisation, but it is shown especially clearly by the relation of individual and population.

Reproduction is not the only function which can bind together the members of a population. We can see the same dependence of types (or morphs as Huxley calls them) upon one another for more efficient protection in cases of Batesian mimicry, and, indeed, it is likely that many if not all examples of polymorphism in plants as well as animals will be found to depend on inter-individual organisation within the population—once the function in which the types co-operate and which binds them together has been discovered. Nor are these ties between individuals to be seen only at the phenotypic level. The studies in population genetics which have become so important during the past decade or so, have shown us that in species whose individuals naturally outbreed rather than inbreed, no individual is genetically self-sufficient. The genic balance upon which depends simultaneously an adequate and fit phenotype in the present and a genotype sufficiently labile to allow change with changing environment circumstances of the future is one which is at least partially heterozygous. Heterozygosity, in its turn, requires in the general case a community of ancestors: it cannot be maintained, in the absence of exceptional and in the end self-destroying mechanisms, by a single lineage ancestry. So the individuals of a population in such species are indissolubly bound together at a genetical level, for only the interbreeding of a number of genetically unlike individuals can secure genic balance and therefore phenotypic fitness in the offspring. The existence of inbreeding species shows us that this genetic tie can be resolved and the organisation broken down ; but owing to their genetical rigidity inbreeders must be short-lived in an evolutionary sense, so that each resolution and

break-down is but a temporary incident. The main stream of evolution depends on populations of interbreeding, genically different and, therefore, genically co-operating individuals—in other words on populations whose member individuals make their different and indispensable genetic contributions and which are, therefore, organisations incapable of reduction beyond a certain point if disaster is to be avoided.

The interdependence of individuals within a population in their division of labour for the better discharge of certain functions is perhaps at its most obvious in social animals and at its most complex in human society. Here we see not one but a whole gamut of functions in which we collaborate, and hence a whole gamut of organisations of which we form parts. Physiologically each of us is in large measure a unit. Reproductively we each form part of a family, which in a larger form is the unit for other aspects of our lives.

In our work and production and in protecting our well-being and interests we form into still larger units, but these units have no one pattern or size even in respect of a single aspect of our needs. Sometimes our organisations are contained one inside another. More often they overlap and cut across one another in greater or lesser degrees, so that individuals with whom we are collaborating in one respect are in conflict with us as rivals in another. The ensuing conflict of loyalties (which are one of the chief means by which we are fitted into our organisations) gives rise to serious problems to which we have as yet found no adequate solution.

This conflict between co-operation as unlikes in respect of one function and competition as likes in respect of another is especially familiar to us in human society. It must clearly occur, too, in any of the populations we have been discussing. The male and female campion or the pin and thrum primrose will, for example, be competitors for ground space, for light and for nutrients, though they must collaborate in reproduction. Indeed, such an internal conflict must characterise organisation at any level, and every system of organisation must include some means of controlling internal competition if it is to persist successfully.

Before proceeding to consider the essential requirements of organisation, one final type remains to be noted : the combination of more than one species into a single system. Such symbiosis, as we call it, has an obvious basis in, for example, the association of bacteria and leguminous plants. In many other cases, however, the bond is less obvious and we are often unsure whether the inference of symbiosis is justifiable or not. In a broad sense, of course, symbiosis must be common, because species must often be essential parts of one another's environment. Indeed, in a sense all agriculture and a growing class of industrial processes could be so regarded. A great deal more investigation will, however, be required into the functional basis and organisational requirements of symbiosis before we can discuss it as a phenomenon in any adequate way.

This very brief survey has shown us organisation at many levels, springing from and bound up with one another, in the discharge of many different functions. They all, of course, depend for their origin and survival on the efficient division of labour among their parts which are themselves organisations at a lower—or to be more precise smaller—level, and which may act independently and hence compete with one another in respect of other functions. These parts may be bound together into the organised whole temporarily or permanently, and by a wide variety of means—morphological, physiological and behaviouristic. Can we discern any common element in these mechanisms of organisation ; any principles which will give us a deeper understanding of biological organisation, not only in the advantages which it can confer and which we have already touched on, but also in its requirements for successful adaptation and evolution ?

It is obvious that any organisation at any biological level must show the property of continuation or recurrence. The way in which nuclear genes, viruses and other basic particles achieve continuity is not precisely known, though clearly it depends on the capacity of nucleo-proteins for reproducing themselves from simple materials. We are here at the border line of chemistry and biology and it is both significant and encouraging that the

President of Section B (Chemistry) is delivering his address on this very subject. We may note, however, that whatever the details of the process, these particles reproduce themselves as wholes: barring accidents, the new gene is a faithful replica when it parts from its parent.

The same is true of the nucleus, and it is made possible by the chromosome mechanism, which achieves the basic requirement that the essential parts of the nucleus —the genes—are kept in step in their reproduction and distribution to daughter cells. In exceptional cases, even at mitosis, chromosomes may be eliminated or fragmented or in differentiated tissues replicated; and at meiosis the chromosomes are both halved in number and readjusted in their allelic content though not, we must observe, in their content of loci. These variants of the mechanisms must not, however, obscure the basic feature that nuclei reproduce as wholes, by the simultaneous and ordered reproduction of their essential parts.

Where the structure acts as its own pattern in reproduction, full continuity of the type is assured from the start—barring, of course, those accidents which would be classed as mutations and structural changes. The cytoplasm, however, is in a different case. Some of its parts, like the nucleus, secure their own reproduction. Others do not: they are built up only in certain circumstances, depending on constitution in respect of nuclear genes and plasmagenes, on environment and on the existing cytoplasmic state. This lack of permanent pattern gives to the cytoplasm that versatility and capacity for change which is its basic property. The changes may be opportunistic as we see them when yeasts 'adapt' to a sugar in their substrate or when we practise the somatic selection which Dr. Jinks will describe in other fungi. Or, when the cell is sheltered from the external environment, the changes may be ordered and cyclical in a way which makes differentiation possible. But in either case the capacity for reacting in a particular fashion is governed ultimately by the fully continuous organs of the cell, above all by the nucleus, as experiment has repeatedly shown us. The organisation of the system and the repeatability of its change is thus achieved by the continuity of one of its parts, a part which paradoxically enough, owes to its very constancy its ability to maintain control and secure the capacity for adaptive and adapted change. At the level of the cell, therefore, we see as the primary division of labour the differentiation of a fixed controlling organ, securing the continuity of the system, from the labile remainder which, responding simultaneously to this controlling action, to external circumstances and to its own constitution, is able to give the flexibility and progressive change upon which successful adjustment and functioning must depend. Upon this initial differentiation all later organisation has been built.

We may note in passing that this system may be disturbed by the intrusion of foreign materials from outside. If, like the viruses, these have the capacity for reproduction and maintaining their own continuity in the medium of the cell, they may be built into the system—often to its disturbance—when they will be recognised as disease, but occasionally perhaps to its advantage when they will be adopted as essential parts. Thus the parts of an organised cell need not all have descended from a common ancestral cell: some may have entered by infection, though once in, to be effective, they must be maintained by their own heredity, now part of that of the cell.

Given cellular organisation of nucleus and cytoplasm, as we have seen it, somatic differentiation has all the basis that it needs. The control of the nucleus must develop in such a way as to replace opportunistic responses by a series of ordered progressions in the changing cytoplasm in large measure divorced from the immediate external environment. This must take place to some extent even in unicellular organisms, but it is not seen in its full complexity until cells themselves take on different functions, and hence forms, in relation to one another. The canalisation of development which is entailed makes two requirements of the nucleus. The genotype must be adjusted to build up certain small differences in the embryonic cells into the greater differences which distinguish tissues. At the same time, however, if the channels of development are to be suitably determined, it

must be able to control and even suppress the consequences of differences initially no more trivial, where these would lead merely to deviation from the types required of the tissues. The physiological means by which these two requirements are fulfilled are many and varied—they may even involve infection of one somatic cell by another—but all must ultimately be mediated by the controlling organ of the cell; and, indeed, we have good evidence that the nucleus is in fact adjusted to the suppression of unadaptive changes in the cells as well as to the promotion of the adaptive.

Populations differ from the organisations we have just been discussing in that they may be propagated characteristically by a gradual and continuous replacement of their parts rather than by a simultaneous replacement occurring as a unique event. This is sometimes of importance, as the new members of the population may become adjusted to their special rôles by their relations to, or by the action of, the existing members. Such, for example, is the case wherever an act of learning is involved. It is especially important in human society, and it occurs, too, in the social hymenoptera.

More commonly, however, the form and function of each member of a population is determined by its own genotype. Males and females, pin and thrum, the incompatibility types and so on are generally determined by specific genes which act by switching development into one or other of the possible paths. Developmentally this involves nothing more than is implied in somatic differentiation, though with the difference that since the individuals come from different fertilised eggs the switching into one developmental path or the other can be done by nuclear genes rather than by the build up of the cytoplasm itself as in somatic differentiation. We thus see the nuclear genotype as the controlling agent of organisation in populations, whose continuity is thereby secured. The only new elaboration is that the genotype must be adjusted to offer and control the necessary alternative paths of development (and must make provision for the switching, where this is genic) instead of merely the one broad path which somatic organisation demands.

The genetic integration which we have already had occasion to note as a feature of populations in out-breeding species is obviously a property of the nuclei and will, therefore, continue so long as the appropriate balance is maintained by the genes. The properties of genic balance are complex and are only now being explored, but we need not consider them in detail for our present purpose.

Symbiosis poses a somewhat different problem. The two or more components of the system must depend on their own genic balances for continuing adjustment, just as do the males and females of a population. Since, however, they are distinct species whose nuclei never share a common lineage, the genic adjustments must depend on external rather than internal relations (as must, indeed, the adjustments of nuclei in the heterokaryon of an imperfect fungus). Also the symbionts must be endowed by heredity with a means of propagation which secures their co-ordinated distribution. Sometimes the one component is the active partner, carrying the other with it, as the fungus is in at least some lichens or as man is in his agriculture; but this need not always be the case. We have, indeed, a vast and as yet virtually untouched field of investigation into the mutual adjustments, and especially the mutual genetical adjustments, inherent in and required by symbiosis.

Human society raises new and special questions of continuity and control. At bottom there is a genetic element, for the members of a community—or most of them—must achieve a minimum genetic level if they are to function adequately as its parts and thereby enable it to function adequately as a whole. But given a genetical minimum, we depend on a process of education (using this term in a very broad sense) to secure the adaptation of the individual and his integration into the community. In some ways this process is better likened to infection than heredity. It clearly depends for success on the gradual replacement of parts in the organisation, it is not confined in its working to the parent-offspring relation, and it operates at a speed which raises problems of adjustment, corresponding to those solved in heredity by the rigid mechanism of the chromosome, to which we have yet

to discover an answer. We have, in fact, a new mechanism of continuity and control in human society, one which is still evolving and with its own internal strains still requiring resolution—as we ourselves know only too well.

Adequate transmission of the controlling agents, whether it be transmission of genes by heredity or of laws, customs and tabus by education, will secure the continuation of an organisation. Other properties, however, are important if we are to understand how organisation, at its various levels, comes into being.

No organisation is immutable : changes are constantly occurring. Changes in genes we term mutation. Nuclei, too, can alter, the chromosomes being affected in many ways—numerically, structurally and functionally ; but being nuclear all of them are capable of being perpetuated except where mechanical considerations forbid it. Cells and tissues may vary in their development and functioning under the influence of altered genotypes, as may the organisation of populations. In short, as the controlling agency varies so will the organisation, and we can see this also in human society whose controlling laws are always under challenge (by reformers as well as by malefactors) and which is, therefore, always liable to change in greater or lesser degree.

Mostly the alterations are degradatory, as we should indeed expect in a highly adjusted system : the gene is no longer capable of efficiently discharging its controlling function, and the chromosome is no longer capable of securing efficient transmission and recombination. In the same way, the changed cell and tissue lose their adjustment and pathological effects, including cancerous development, follow. Such changes would be disastrous if they were not eliminated by selection. Where the changed entity is but one of a number, it may go under in competition with its fellows inside the system as must happen with many altered particles in a cell or with, for example, inefficient sexual types in a population. The part is replaced without sacrifice of the whole. Or the selection may be brought about by a resulting distortion of efficiency of the organisation to which the entity contributes and which then fails in competition with its fellow organisations. The part then causes the failure of the whole, whose elimination carries with it elimination of the faulty part, as is seen with faulty genes or with cancerous individuals.

Occasionally, however, change will lead to a more efficient functioning and then the altered entity will be favoured by selection of one type or the other. The significant variation is that of the continuing parts of the cell, especially the nuclear genes. The selection may be at all levels and may, indeed, depend on an interaction or a conflict of levels, as when a cell changes so as to secure an advantage over its fellows but only at the expense of the soma of which it should form an integral part, or when a cell is sacrificed for the benefit of the soma. But given such variation and selection, as well as transmission, we have all that is necessary for the continuing adaptation and evolution of organisations at all biological levels. Our genetical evidence is unanimous that all the complexity of biological organisation and its control has arisen in just this Darwinian fashion, though many of the intricacies, especially of gene action and adjustment and of the selective processes, remain to be elucidated.

Can we say the same of the organisation of human society? I believe that the variation we can see in our society and the selection that we can infer from the past enables us to do so, though a full statement of my reasons would be too lengthy for presentation now. The selection, however, must often, especially in the more distant past, have been by competition external to the individual society, rather than by adjustment within it. Such external competition whether by war, cold or hot, or any other means, is a painful process rightly distasteful to us all. Furthermore, as societies grow in size and dwindle in number, external competition necessarily becomes more wasteful, more unpleasant and indeed less certain in its consequences, for it becomes increasingly subject to the vagaries of drift. We must seek to replace it by controlled internal competition and smooth internal adjustment. In doing so, however, we must remember that the achievement of adjustment by internal means is a complex process, which has not succeeded in

ousting selective elimination at any biological level. To bring about the complete substitution of the one for the other in our affairs will not be easy. It will require an understanding of ourselves which we do not yet possess, but for which we must constantly strive.

I have tried to show that the same problems arise and principles apply at all the many levels of biological organisation : principles of division of labour, of relation of organisation to function, of transmission, variation and selection : problems of interaction of the levels, of conflict in the requirements of different functions, and of control to resolve this conflict. We have already achieved some understanding, incomplete though it may be, of these principles and problems at biological levels. Using this knowledge, and applying it to human affairs—while paying full regard to the different type of control, resembling as it does infection more than heredity, and the greater complexity of the system with its plethora of conflicting functions—may we not hope to achieve a better understanding of our own problems? One thing seems to me in any case to be certain. Our society is but another level of organisation superimposed on all the biological systems below it. To flout the principles common to them all, and to shut our eyes to their interactions could lead only to disaster.

THE EDUCATED MAN IN 1955

Address by

J. F. WOLFENDEN, C.B.E.

PRESIDENT OF SECTION L

WHAT is education for? Why do the nations of the world spend so many millions of pounds, dollars, francs or kronor every year on teachers' salaries, school buildings and educational equipment? More important, why do we, and hundreds of thousands like us all over the world, give our lives and all our energies to this service?

Of making many books in answer to these questions there is no end; and I certainly do not propose to add to the number of slim volumes, with engaging titles like 'Education for Life,' 'Education for Leisure' or—if one of my more distinguished predecessors will forgive me —'Education for a World Adrift,' which you all possess. It is not my purpose this morning even to try to answer these questions. But I cannot altogether ignore them. For even a comparatively superficial examination will reveal what seems to me to be one of the fundamental dilemmas of all education. It is, in its simplest terms, this. On the one hand, all education is rooted and grounded in the past. Inevitably it reflects the history and the pattern of the civilisation of which it is a part. Indeed, I have more than once read that it is the function of education to be conservative, to fortify and preserve the culture of a tribe, a people or a class. So in this sense, and in this part of its purpose, education looks back and recognises its obligation to the past. On the other hand, it is part of our duty to look forward into the future. For, at the lowest, it is in the future that the young people whom we are educating will spend their lives, while at the highest it might well be held to be our responsibility to ensure that they, in that future, will make their world a better place than the one we or their grandparents have known. Unless there is some deliberate intention of this kind, how can the human race make any progress at all? We are, in short, always working for the future, although exactly what that future will be like we cannot ever, in practical detail, know. So we are always to some extent working in the dark, guessing, as intelligently as we can, walking by faith and not by sight, building, in fact, for a future we cannot see.

There are of course several other awkward dilemmas and paradoxes about our educational ideals and objectives; but this particular one will give us quite enough to think about for the time that is at our disposal this morning.

It is easy enough to see, in the educational systems of earlier days, on which limb of the disjunction the emphasis fell. Sparta and Rome may, on a broad view, be taken as typically conservative, with no impulse towards change and a deliberate training of young people to maintain the habits and standards of their forefathers. Much the same, I guess, is true of education in England until comparatively lately. In the days of ecclesiastical domination that was natural enough. In the period of the Renaissance the new learning did in paradoxical fact turn the eyes of men's minds backwards, behind the Middle Ages and behind Christianity itself, to the golden culture of Athens. In the first half of the eighteenth century the fixed and stable education on a classical pattern reflected the whole pattern of a static, not to say stagnant, society.

But at some moment there came a break. Or, more likely, it gradually happened, from some combination of unenumerated causes, that the interest in the future became stronger than the interest in the past. Perhaps the apparently limitless prospects of expansion in countries beyond the seas was

the main factor ; perhaps it was the realisation of vast new economic possibilities here at home ; perhaps it was the more immediately relevant impact of scientific discoveries and intentions. Any one of these new phenomena alone might have upset the traditional backward-looking pattern. In combination they were irresistible, and from the middle of the nineteenth century at any rate the future takes its place alongside the past in the interests and objectives of the educator. Over the past fifty years it may be that this emphasis has gone far enough—even perhaps too far ; on that point I am not, I think, required to pass judgment. At least, it has come about that the majority of us, I should guess, are as educators more interested in the future than we are in the past, more interested, if you like, in speculative building than in the preservation of Merrie England.

We find, naturally enough, that each civilisation and each age has its picture of the typical ' educated man,' corresponding to the broad pattern of the age and civilisation itself. I know, of course, that these generalisations invite—and certainly admit —exceptions ; but, nevertheless, I believe that there are valid generalisations to be made. The physical fortitude of the Spartan, the rather solemn but extremely efficient reliability of the Roman, the orthodoxy of the thirteenth-century Catholic, the gay and sophisticated paganism of the Elizabethan Englishman, the leisurely classicism of Addison or Pitt—each of these reflects its place and time ; and although there are, as I have said, obvious exceptions, they can, I think, be claimed as typical of the pattern of their respective civilisations. From the traditional Spartan boy, silent and impassive while the fox gnawed his vitals, to Philip Sidney ' naturalising the classical metres into English verse ' and dying as the perfect type of the ' officer and gentleman,' or Burke rolling out his Demosthenic periods in the House of Commons, each represents the current educational ideal of his generation. Broadly, there is discernible a typical ' educated man ' to correspond to the day and age and civilisation. Is there anything comparable which we can put forward as representing what we here and now, in Britain today, regard as our contemporary ideal ?

There are those, and they deserve our admiration, who still believe that the prime distinguishing mark of the educated man is his understanding of human nature. This is not necessarily a ' conservative ' view, though it is often found in conjunction with a sympathy for the past. At bottom it is a recognition, indeed an assertion, that the most important feature of this planet is the fact that human beings have lived and do live on it. Those human beings, each a unique blending of spatio-temporal body with eternal spirit, are what give significance to the whole process of history ; without them the whole choir of heaven and furniture of earth would be meaningless and valueless. It is natural that those who hold this view should be more interested in history, literature, philosophy, economics, the spheres in which human motive, emotion, thought and action show themselves, than in the more abstract and impersonal branches of study and enquiry. It is not very helpful to call them humanists, for that word has become protean in its meanings ; but that is essentially what they are. Nor is it helpful to emphasise their normal interest in the past, for what causes that is not so much their devotion to the past in itself as their use of the past to illuminate human conduct in the present. Their study is concentrated on human beings because they believe that human beings are important, wherever or whenever they lived. That value-judgment, of the supreme importance of the human individual, underlies their whole outlook ; that is the essence and substance of their attitude, while an interest in any one period or civilisation or manifestation of human activity is, in the proper sense, accidental. Their fundamental study is the endless variety of human motive and human response, the relation of a man to other men, to institutions, to his environment and, sometimes, to God.

The pattern of education which would correspond to this outlook is easily inferred. On the one hand it will involve a study, as deep and prolonged as may be, of human activity in the past, as recorded and revealed in history and in literature. To this extent it is ' bookish,' since it is from the written and printed record that this insight into human nature can be most

conveniently and comprehensively acquired. On the other hand, such an outlook ought to lead to a deep and practical immersion in human affairs, personal, social and political. For it is in real life that real life can best be studied.

On these lines, it is not difficult to see why an education of this kind was regarded as appropriate, down the centuries, for anybody who had a part to play in public affairs, whether as a statesman in the national life or as a more local county magnate or justice of the peace. We mislead ourselves if we describe it, at any rate until very modern times, as a ' classical ' education, in contrast with some other more modern form of upbringing. That it looked back to the history and literature of earlier times, especially of the times called ' classical,' was what I have called accidental ; substantially the education was humanistic, because it was concerned with human beings. And it was fertilised by the daily contact with practical affairs, in Parliament or the magistrates' courts, so that it never became, in the really educated man, merely bookish or arid. The past enriched the individual's mind and gave him a pattern of experience ; the pattern was filled out by the diversity of daily events. The man who had no such daily contact with human affairs might well become more learned than his more practical brother ; the busy daily practitioner might become more skilful than the scholar ; neither was a fully educated man unless he both possessed the wealth of second-hand experience which his studies gave him and also in his own daily life had first-hand personal experience of human actions.

Well, how does this picture, oversimplified as I know it to be, look, from the standpoint of the year 1955 ? Of the many comments which might be made upon it, I propose to concentrate on two. And I will take the less important first.

The first and most obvious comment, I suppose, is how much it leaves out. Granted that human thought and action are important, perhaps the most important phenomena of this planet, there are other phenomena as well which deserve and demand our interest. And however wide you stretch the bounds of what is human to include, besides literature, history, psychology and economics, such studies as geography, medicine, agriculture and law, there remains a whole realm which is not primarily human at all but which cannot, for that reason, be left out of the concern of the educated man. Natural science, if those words be interpreted as widely as we have been interpreting human affairs, will leave a very big hole if it be omitted entirely from the picture.

We must not, as I hinted a few minutes ago, be misled into an antithesis between a ' classical ' and a ' modern ' or between a ' literary ' and a ' scientific ' form of education. The fact that such a disjunction is a normal and recognised form of speech shows how far we have fallen away from an organic and coherent pattern of education. It may, indeed, show several other things as well. It shows, I think, that man has developed, for some reason or other, a curiosity about non-human things in addition to his desire to know about other human beings. It shows that some people regard these things as important, as important as (perhaps more important than) human beings. And it shows also that there has grown up what can only be called a ' disinterested ' desire for knowledge,that is, an enthusiasm for finding out about things although the information so acquired may be quite useless in any application to human affairs. There are a good many paradoxes hidden in the last few sentences, and I have time to do no more than merely point them out. It is odd, or so it seems to me, that human beings, with human minds, should regard some things outside all minds as being more important than those minds themselves ; I am not quite sure how this chimes in with the traditional ' cosmological ' proof of the existence of God. It is odd, again, that ' disinterested ' knowledge should ever have occurred to any creature as earthbound as man. It is odd, in the light of our recent educational experience, that it should be the scientific rather than the humanistic form of education which should originally be called disinterested ; for in our day and generation it is normally the classical pattern of education which is regarded as useless and treated with reverence or contempt in consequence. Finally, it is odd that the very studies which once seemed so innocent of human content

are now the very ones which threaten to have so cataclysmic an impact on our civilisation.

For whatever the humanist may say—if we may permit ourselves for a moment to talk in terms of this disreputable disjunction—nobody can, in 1955, ignore the presence, in our lives and in our education, of scientific thinking and its consequences. Here is a final paradox which in its way is the oddest of all. The function and purpose of the humanist education was that it should be applied, in ordinary daily living, to the motives and actions of human beings. Yet today this kind of education is almost universally regarded, even when it is being practised to the full, as remote from the dust and heat of the day, while scientific knowledge, originally remote from daily human affairs, is now applied to practically everything we do.

The simple and platitudinous fact is that during the lifetime of some of us the external conditions of daily life have changed more radically than within any other half-century in the history of this planet—and I should not be prepared to assert that those changes in the external conditions of living have had no effect on the deepest and inmost activity of the human mind. Doubtless thousands of generations before ours have proudly said that they were living in an age of transition ; I expect Eve said it to Adam. But however hard we try to preserve a modest sense of proportion we cannot possibly escape the judgment that this is a generation of change which is rather out of the ordinary. All this, as I have said, is platitude. But the implications of these truisms for education have not, I think, been seen as quickly or as vividly as the scientific advances have manifested themselves.

Let me put it on a very humble practical level, which I hope you will not find offensive. I fancy that most of you would regard yourselves as pretty well educated. I used so to regard myself, especially perhaps when I had emerged, in skin-of-teeth triumph, from the Final Honour School of the More Humane Letters at Oxford. But the longer I live the more uneducated I feel myself to be. Now that is not simply because I was brought up on the classics, on literature and, in general, bookishly, though I well know that partisans of other forms of education might say that it was. The fact is that if a man is going to deserve to be called educated in 1955 there are all kinds of things he must know that I have never learnt. It goes further than that. It is not just a matter of acquiring information ; I dare say that I could do that, given the time. There are all sorts of ways of thinking which were foreign to my upbringing (and, perhaps, to yours) but which are part of the educated man's indispensable equipment in 1955. It is difficult for me even to say what they are, because I can grasp them only dimly. But I am aware, for instance, that certain modes of thinking which derive from genetics or from modern physics are simply foreign to my own habits of mind. It would be merely silly, and disgracefully obscurantist, to say that, because they are alien to my ways of thinking, they are queer or new-fangled. And I think it would be equally wrong to pretend that they were merely part of somebody else's ' subject ' but no concern of mine. Just as, mechanically, we live in a world of motor-cars and aeroplanes and have to adjust ourselves, physically, to the existence of the internal combustion engine, so in our mental habits and attitudes we have to take account of ways of thinking which are to some of us no less novel, exciting and exacting.

Do not misunderstand me. I do not mean that in 1955 the scientist is automatically a better educated man than the non-scientist. There are things which he must know, and attitudes of mind which he must have, if he is to be called educated. And it is not necessarily the case that his studies provide him with all of these any more than my upbringing provided me with them. He may be a very learned, ingenious and modern-minded scientist ; but he may also be illiterate and incapable of historical imagination or of human insight. But what I do mean is that I am scientifically illiterate, ignorant not merely of a great many scientific facts but apparently incapable of some of the concepts and modes of thought which are appropriate, if not essential, in a scientific age like the present. The truth is that in our present-day civilisation more is required than just the human without the scientific

or just the scientific without the human. If we are going to be able to behave as educated persons in the present age we need both—and a very severe demand it is. In short, if I may be allowed to parody one sentence from a great philosopher, science without humanity is void ; humanity without science, blind.

Another thing I do not mean is that the scientist should be regarded as the man who should decide our policies for us, in affairs local, national or international. The fact that he understands all these things that I do not, or uses these modes of thought that I do not, does not in itself qualify him to take the place which the educated man in history has taken. It seems to me to be sadly wide of the mark that people should deplore, for instance, the absence of scientists from the House of Commons, or demand that they should be arbiters of international foreign policy. This is a regrettable misunderstanding of the nature of their training and of their skills. The very fact that for the purposes of their own studies they must largely ignore human factors is not, as some people seem to think, a qualification for making decisions which may affect the lives of millions. They must, indeed, at all stages and at all relevant points, make perfectly clear what will be the consequences, in scientific terms, of any decisions which are contemplated. But it is very nearly superstitious to suppose that they are particularly fitted for political decision, and it is certainly irrational to suppose that any such skill as they may possess is derived from the nature of their upbringing. The greatest and most skilful of them assert this themselves, for the greater they are the humbler they are. But some journalistic assessments have transferred to the scientist all the arrogance of the earlier humanist and a bit more besides. For my part, I humbly welcome and regard with some awe both the scientific thinker and the skilled technologist ; I hope there will be many more of both. But I do not think that they possess all knowledge, either separately or collectively ; and I do not think that the simple multiplication of technologists will automatically increase the amount of wisdom in the world.

There I must leave this part of my theme. I called it, earlier, the less important of the two major points I wanted to raise. It is important enough, in all conscience, to recognise how relevant the scientific attitude of mind is to any description of an educated man in these modern days ; and I do not at all wish to minimise the need for amending, to include it, any lingering exclusiveness of a conservative kind. For if, as I have suggested, education ought to look forward as well as backward, then we must clearly recognise that in the dawn of a technological age we must not leave our children to face the future with blinded eyes.

I turn, then, to what I venture to regard as an even more important fact about education in our changing society.

So far we have been thinking of education and the educated man in relation to what must always, I suspect, be a minority. Whether we are thinking of the humanistic tradition or of the newer scientific contribution, what I have said so far can be relevant only to a comparatively small part of the total population of any country. The man of affairs and the man who handles with familiarity and ease the scientific concepts of the twentieth century is an exceptional person, at any rate in the sense that he possesses, either by natural endowment or by acquisition, more than the average amount of facility in dealing with people and ideas. He belongs, if I may risk a dangerous word, to an aristocracy, not of birth or of wealth or of character but of intellect or of influence. If that sounds too high-falutin' a description of him, let us rather say, with as much bathos as you like, that he belongs to what we must not call ' the top twenty-five per cent.', to that part of the population which goes into the grammar-technical 'bracket' —or would, if selection machinery were infallible.

At this point we must be sternly on our guard against intellectual pride. Let me put in its most naïve form the question I have now to ask. Granted, the question runs, that you have said something about the educated man in terms of the grammar-technical stream, what about all the rest ? Are you excluding them from all possibility of being called ' educated men ? ' Is there, or is there not, some complementary picture which would apply to

what we barbarously call the ' secondary-modern-school-type ? '

Before we begin to answer these questions, let me call attention to one rather remarkable fact. It is this, that it seems to me to indicate a revolution in educational thinking that we can ask these questions at all. It is really only very lately that we have begun to think at all in terms of educating everybody. The ' aristocracy ' to which I referred just now was, until a short time ago, a very small and closely circumscribed fraction of our educational field. Or, to put it another way, it was the whole of our educational field, and nobody bothered very much about anything outside it. I know, of course, that the apprentice was taught by the master-craftsman ; I know that there have been Cabinet Ministers (and very good ones) who had never had a grammar school education ; and I know, very well indeed, that intellectual ability and nobility of character do not always (to put it mildly) go hand in hand. All this, of course, is true. But it does not affect my point. For my point is that it is only very lately that we in this country have consciously adopted an attitude towards education which can properly be called democratic. I do not mean, by that adjective, that we have only lately opened to talent, instead of keeping confined to birth or money, the enjoyment of the kind of education I just now called ' aristocratic,' I mean something much more startling, namely, that we have lately come to recognise as qualifying to be called ' secondary education ' forms of study and learning which until a few years ago nobody would have dreamed of calling by that name. This is the most remarkable single fact about education in this country for at least the past half-century, and it deserves a minute or two of our time and attention.

When Disraeli said, in the House of Commons 81 years ago, ' Upon the education of the people of this country the fate of this country depends,' I wonder if he realised the full implications of what he was saying. Personally, I rather suspect that he did, and that the author of ' Sybil ' was here performing the very act which he had attributed to Sir Robert Peel thirty years earlier, of catching the progressives bathing and walking away with their clothes. I do not mean that he foresaw our present-day educational system, with its highly complicated details. But I believe that by ' education ' he meant something far more than the miserable minimum of elementary teaching which even the liberal-minded among his contemporaries thought was good enough for the masses. We have moved a long way since those days ; we have in fact moved up to the present day, and now we had better try to answer our questions, keeping at the back of our minds this tremendous revolution in educational theory and practice.

Is there, then, to return to our question, any picture, complementary to that which we have sketched for the grammar-school type, which will apply to the secondary-modern type ? Or is the title of ' educated man ' to be reserved for the man who can combine the humanistic tradition with the scientific ? (We must remember, incidentally, that by this criterion there are very few of us who can justify our claim to the title today.)

Now let me say at once that for my own part I can find no such complementary picture. I am afraid that may sound shockingly undemocratic, or perversely obscurantist or even academically snobbish. But whatever else I may or may not be being this morning, I am trying to be honest. And if I am to be honest, that is my frank answer to the questions I have put. Let me try, in the time that remains, to give my reasons.

I believe, unrepentantly, that to be called an educated man a person must be able to move with some facility in at least two spheres. He must be able to appreciate, in something like perspective, the achievements of the human race, in government, literature, art and social living. And, secondly, he must be able to move with some facility in the realm of abstract ideas. If you ask me why I so dogmatically choose these two criteria, I can only answer honestly and for myself, in some such way as this. I believe that for a human being, like you and me, the operations of human beings, whether in social action, or in creative performance, or in straight hard thinking, are the proper subject-matter of education in the fullest

sense of the word. I cannot, at any rate, believe that a man who is without these can really be called educated. It does not matter which particular part of the story of the human race he finds most congenial ; the Indian, the African, the Russian, is just as much entitled to qualify on these terms as the member of the more narrowly Western civilisation, which seems to be moving its geographical centre westwards across the Atlantic. Nor does it matter whether the field of abstract ideas in which he moves is mathematical, scientific, legal or more technically philosophical. But if it is true, as I believe it to be, that conceptual thinking is a specifically human activity, then I cannot but think that some ability to frame concepts, to deal with abstractions, in short to think, is an essential element in the description of an educated man. Obviously there will be kinds of education, and degrees of education; but for myself I must admit, not without some embarrassment, that these are for me the fundamental criteria, and that therefore if they are not, in some degree at least, satisfied, I cannot recognise that I am in the presence of an educated man.

I do feel embarrassment about this, because I know it must look as if I am simply cheering for the particular form of education in which I was myself brought up. I hope it is not simply that—though if it is, I find that more pardonable than the prevalent practice of kicking down the ladder by which one has climbed up, often to a position of leadership in one branch or other of the national life. If we are to attend to the strict meaning of words, I am afraid I cannot believe that the type of mind which I have, in shorthand jargon, called the secondary-modern type, can really aspire to the title we are discussing. That is not, as a matter of fact, anything to anybody's discredit. For one thing, presumably it does not want to. For another, there is no particular moral worth or human excellence to be attributed to the possession of a particular type of mind—any more than there is to the possession of red hair or small feet. Educated men are often rather unpleasant people, morally and aesthetically; but they do possess certain characteristics which, in their own line, are notable. And these particular characteristics the secondary-modern type does not possess.

Now I know I shall be told that, as I have myself hinted, there are various different kinds of being 'educated,' that there are diversities of gifts but the same spirit. I shall be told that it is reactionary to question the equal validity of various forms of education. And I shall be told that I am denying the whole notion of democracy which I mentioned a few minutes ago. Let me look for a moment at each of these criticisms.

First, let me concede at once that there are various forms of education, diversities within the one spiritual whole. But let me make two observations. The first is this. The reason why there are various different forms of education, the very point and purpose of this diversity, is that they are deliberately intended and designed to cater for different types of children. The great advance which is codified in the 1944 Act, having been for long before that a commonplace among educational practitioners, is that just because children differ in age, ability and aptitude, so the kind of education which is given to them should differ from one to the other relevantly and appropriately. It is not the case that we have invented a few different kinds of education and then tried to find corresponding differences between children ; we start from the fact that children differ and design different types of education to fit them. My second observation on this point must be dogmatic. I find it terribly hard to attach genuine significance to such words as 'learning through his hands'. That, doubtless, is my fault. But all this sort of phraseology, whether it is applied to school life or to work in a factory, seems to me to be either metaphor or an attempt to introduce a familiar concept by the back door with a flavour different from its accepted one. This, I have admitted, is dogmatic, and it may sound unsympathetic. It is not so meant ; but it is meant as a plea for straight thinking and plain speaking.

The second objection I anticipated is that it is reactionary to question the equal validity of various forms of education. Here I think (indeed I hope) that I am flogging a dead horse. When we are required to recognise as equally valid

various different things, or to regard them, in historic phrase, with parity of esteem, what exactly are we being asked to do? If we are being asked to agree that we are talking about several different things, each in its own way as good as it can be, that is one thing. If we are being asked to agree that several different things are all equal to each other, that is another thing. The former of these propositions I would whole-heartedly endorse; the latter I cannot, in this field at any rate, accept. The whole point, as I have just said, about our different types of education is that each is the right one for the right type of child. The question whether they are all equal seems to me to be, strictly speaking, an improper question. A grammar school education for a child for whom it is inappropriate is the wrong kind of education. A non-grammar school education for a child for whom a grammar school education is appropriate is the wrong kind of education. Which is the more wrong, and for which, I do not know, and I do not know how anybody can know. They may, for all I know, be equally wrong, just as the right kinds for the right children may be equally right. But to introduce the notion of equality in what is not a quantitative but a qualitative question seems to me to be just darkening counsel.

Thirdly, I accused myself of sounding undemocratic. But I find that the notion of education in accordance with the 3A's, ability, age and aptitude, is precisely the most democratic system which has so far been devised. It gives the right thing to the right people. It does not give the same thing to everybody, because it explicitly recognises, as I have said, the differences between people. So it is not egalitarian. But we have by now, I hope, grown out of the brief period in which we confused democracy with egalitarianism and fondly thought that if everybody had the same education, or the same income, or, I suppose, the same number of children, we should be achieving democracy. There is nothing undemocratic about differences. The offence comes when extraneous adjectives, of approval or of condemnation, are introduced into a field where they do not belong. That is undemocratic. And it is therefore undemocratic to attribute moral excellence or civic worth or economic value to the educated man, whether as I have described him or not. But inside the field of education to discriminate between one kind of achievement and another is indeed un-egalitarian but it is not undemocratic.

So I must repeat, still with embarrassment but with that much attempt at explanation, that I feel bound to confine the title of educated man, for what it is worth, to those who happen to possess, by original endowment or by acquisition, some proficiency in human affairs and in the management of abstract notions. I do not think that this is undemocratic; and I do not think that being an educated man involves any particular merit or worth in other contexts.

May I now come back to where I started? I believe that for those of us who are concerned with education the future is more important than the past. So let us, standing uncomfortably on the knife-edge of the present moment, try to look a little way into the future, helped and guided by what we know of the past.

The tremendous movement to educate everybody, up to the limits of their abilities and aptitudes, is in its very earliest days. So far, we have agreed that there shall be a secondary stage in the education of all our children. It may not last as long as some would wish—though it may, on the other hand, last longer than some would wish who undergo it—but it is there. Our next task, obviously, is to find the right forms of such education, the kinds appropriate to each one of the children who are to have it. At present, clearly, we have only rough approximations to this. For I imagine nobody would be content with the view that some seventy per cent. of all children should be lumped together as broadly similar under the omnibus heading 'secondary modern.' What sort of discriminations ought, in the future, to be introduced among this group? Is there any hint here of a new concept of the educated man?

Tentatively and with diffidence I would suggest that one of our dogmatic criteria may well be satisfied. I can see that there is a real possibility of bringing into the education of everybody that acquaintance with human affairs which seems to me to be an indispensable qualification. It will

be at a different level, and it will have a different bias, from those which were part of the humanist tradition. For it, naturally enough, directed its attention to the behaviour of what had in earlier civilisations been the governing class ; naturally, since it was the education of the governors rather than the governed which was in point. There will always be governors and governed, by whatever names we call them and however unegalitarian it may sound to say so. But there is a real possibility in the future of the development of a familiarity with human affairs from a quite different point of view, concerned not with governments or policies but with the shopping basket and the bus queue. There could well be a growing un-aristocratic culture, based quite explicitly on the great majority of a population, working out in its own appropriate way something analogous to the aristocratic humanist interests of the past and present. I do not believe that we have achieved this yet, and I think it will be very difficult to achieve ever. But it would seem to be one of the most exciting possibilities of the next fifty years.

I find it harder to believe that the second dogmatic criterion, of ready familiarity with abstract concepts, could ever be satisfied by the majority of any population. But here there is an alternative. It could happen, quite soon, with the incredible advances which technology is making, that the emphasis in human thinking may move away from conceptual thought to something else. Quite what that would be I cannot myself foresee, if only for the reason of my own incapacity in this form of thinking. But I can see that it is possible, and that without the realisation of the grimmer horrors of current science fiction there may somehow develop a form of sheer thinking which goes beyond the mathematical as the mathematical has already gone beyond the verbal. I still find it difficult to believe that this would ever be within the grasp of the majority of the population. Perhaps I may be excused from following further fantasies of this kind. I am not required to produce a blue print of the educated man in 2055, or even in 1984. It is difficult enough to decide what one means by an educated man in 1955.

THE IMPACT OF EDUCATION ON FARMING

Address by

W. B. MERCER, C.B.E.

PRESIDENT OF SECTION M

IT is natural that an association for the advancement of science should from time to time pause to take stock of the provision being made for technical education. I am deeply sensible of the honour the Section has done me by electing me to the chair for this meeting and I hesitated before selecting for my address a theme so near in title to those around which Dr. Charles Crowther at Liverpool and the late Professor Comber at Newcastle wove their observations and reflections. But the subject is a large one ; I shall endeavour, by looking at it in the round as well as in section, to avoid invidious comparisons.

Agricultural education was born of great tribulation in the latter years of last century. The infant was not greeted by practising farmers with great acclaim. They were far too much occupied with the problem of making a living. The Royal Commission of 1893 presented a series of reports each vying with the other in gloom. It was hardly to be expected that a body appointed to ' enquire into the present depression in agriculture ' would present a glowing picture of the nation's farming, but Parliament must surely have hoped for something a little more helpful as a result of their labours ; for their recommendations read today like those of a group grappling with problems beyond them.

Not many farm accounts were available in those days, but what they could get showed a distressing state of affairs. We were passing over from a subsistence to a money economy, but subsistence was still a major form of return ; for which reason, primarily, the Commission found that smallholders were doing better than larger farmers. Toil, then, was the only obvious answer to low prices.

The trader was not a conspicuous figure in the farm scene. The rank and file of farmers were without leaders, and for the most part mute ; three out of every four living isolated lives, bowed with toil and sunk in tradition. One misses the figure of the man of original mind, the visionary, seeking out a new line for himself.

The Commission recommended that steps be taken to improve rural education—though they did not place the item very high on their list and it is doubtful whether they were thinking specifically of technical education.

Education, we know, is the cultivation of a habit of mind, but a technical service must rest on a solid foundation of ascertained fact. And this body of information was at the time really very small. True, there were giants on the earth in those days. Lawes and Gilbert still lived ; thanks to them empiricism had given way to scientific method. But two men could not create a science of farming ; they achieved marvels in laying the foundations.

One can get a fair picture of the extent of the educationists' armoury by looking at the early text-books. With one brilliant exception they consisted of the elements of soil chemistry, derived largely from Rothamsted, and recitals of routine work on the farm. The exception—and oddly enough this also derived from Rothamsted —was Warington's *Chemistry of the Farm.*

Mendel's papers were still buried in an obscure corner of a foreign field. In the domain of animal husbandry Wolff's analyses were available ; little practical use could be made of them. Kelner's basic work had scarcely reached fruition. The scientific man could therefore do little more than limp after and try to interpret the feeding methods of successful practical men. Wisely, perhaps, he concentrated on crops and fertilisers. The discovery of fertilisers was an achievement

comparable almost with that of fire. Of them at least he could claim to have special knowledge ; in them he commanded a weapon of immense power.

Things improved somewhat in the first decade of the present century, but everyone will, I suppose, concede that the modern state dates from the Liberal era which opened in 1906. Equally our educational policy stems therefrom, the most important single Act being the Development and Road Fund Act of 1910. We were fortunate that a man of Hall's vision was on the scene at the time. The early educationists had been all-purpose men, attempting at one and the same time to be academic lecturers, teachers, advisers, experimentalists, even research workers. It is true they had not all our shackles and inhibitions. They lived before ' student ' had inspired Hall and one of his young disciples to impose the doctrine of experimental error upon them ; so that Somerville could unconcernedly lay down an experiment destined to live for more than half a century without a replicated plot. It was plain that the field was too wide for one set of men ; moreover, the qualities required in one type of worker were different from those in another. From now on the field divided into research, advisory work, and teaching. In essence the scheme assumed that research, organised on a subject basis and already demanding teamwork, would discover new facts while advisers of various types would develop practical applications, the researcher analysing, the advisers synthesising farm practice. The exact means whereby the results of research would be translated were not very fully worked out, though it was evident that the research stations themselves, the universities and colleges and the county councils, might all be called on to take part, and that field experimentation would figure prominently. But the size of the job was probably underestimated ; so also was the complexity of the work, once it advanced beyond simple fields trials. It was clear that a heavy burden would fall on the county councils, charged with the triple duty of public education experiment and private advice to farmers.

I must digress for a moment to say a few words on their teaching work. Organised instruction of adolescents cannot be expected to have immediate repercussions on practice. It is long-range work, bearing fruit only as students attain manhood and the management of farms. While serving local ends and coloured by local climate of farming opinion it derives much strength from non-farming elements on the local authority. There are not a few counties in which vision and drive in agricultural education have come quite as much from the enthusiast on general education as from the representatives of farming.

In the early days the Farm Institute was regarded by the general run of farmers as an alternative to rather than a supplement to home training and in consequence a high proportion of the students came from non-farming or even non-rural homes. There have been encouraging signs of a change in recent years—at one of the institutes I visited recently nearly half the students were sons or near relatives of farmers who had themselves been students. (The experience of colleges has been very similar. Even now, taking the country as a whole, I doubt whether 50 per cent. of the places in our educational establishments are occupied by farmers' sons. They all tend to attract, in the first place, the man who is looking for paid employment.) The farm institutes strive to get the best of both worlds by dividing their day equally between the classroom and the farm. What they have to reach is no esoteric learning, but modes of thought with which a great proportion of the adult farming public are already familiar. Teaching revolves around the farm so closely that the teacher has to be on his guard against becoming so immersed in its details as to lose sight of general principles in particular applications—for farming is a peculiarly personal experience.

There is a natural affinity between the educationist running a farm and the pioneer farmer : outside contacts tend to be mainly with the upper ten. When the institutes were founded it was expected that they would devote a good deal of time and effort to experimental plot work : but with a few notable exceptions things have not worked out that way. Rather have they evolved as farms devoted to studies of management problems, sometimes

comparative, but more frequently trial-and-error studies whose results are expressed in enterprise returns, cost records playing a valuable part.

But I must return to the situation when the research and educational scheme was devised. We did not then realise how interlocked were technical progress and material prosperity. Before this could be realised the nation had to unlearn some of the canons of free trade and develop a new outlook towards the farmer as a consumer. Experience was to show that a prosperous farm population offered a market worth cultivating and that ancillary trades such as fertilisers, foodstuffs, machinery might well find it sufficiently attractive to justify heavy outlay on research and invention. We could not foresee the rise of public corporations and marketing boards, nor their possible influence on technical progress, though it was destined to be weighty. Least of all did we realise the psychological effect that prosperity might have on farmers themselves, on their willingness to adventure. I doubt whether we realised that prosperous farming would be likely to attract capital from other industries; it was certainly not clear that in good times pioneers might emerge from within the ranks of farmers. We accepted chill penury as part and parcel of the agricultural way of life and comforted ourselves with the reflection that we possessed a noble rage.

An ordered prosperity was in due course to come as a feature in the Welfare State. Agriculture is now recognised as a major component in the nation's economy, a component in whose welfare the State is as deeply involved as any individual. For the modern State stands committed to such an enormous defence and social policy that it must preserve the ability of its members to pay taxes. A very complicated situation arises when direct grants or subsidies are introduced, since part of them reappear as marginal profit of which the State takes a goodly share. Investment in the land, i.e. privately owned land, then takes on a wholly new appearance. Certainly once the State assumes direction 'we are all in it together'—a fact not always recognised perhaps when long-term security is under discussion.

The method of evolution of our policy—typically pragmatical—lies outside the scope of this address. Only certain features call here for mention. At all periods of history we have, as a people, rendered at least lip service to the idea of efficient cultivation of our soil, but at no period has the question of the number of men it is desirable to have employed thereon been squarely faced; we have assumed a position midway between that of the older Continental countries, to whom a high rural population has appealed, despite low output per man, and that of the younger lands with space to spare who tend to measure progress by output per man regardless of yields per acre. Our economy calls for constant improvement in both directions. It came, I think, as a surprise to everyone to find that a nation's output from so complicated an industry as farming can be closely defined, and even estimated in advance. Thanks to twenty years' work by university and State economists, we found when the need for their records arose that a national agricultural budget was a perfectly feasible project; indeed, ever since State direction started there has been a close fit between forecasts and actual figures.

With State direction, and probably arising largely therefrom, there has come a new exactitude into the agricultural world, a much wider understanding of both the accountant's and the economist's concepts. We have moved very far from the days of subsistence farming and the bag of money in the tail coat pocket.

The problem of directing a national policy which of its nature must change with changes in world affairs, and, at the same time, being absolutely fair to all types of producers, is more intractable since in modern times division of labour and interchange of goods between farmers has been carried to such great lengths that a large part of the output ultimately reaching the market has passed, in one form or another, through the hands of two or more producers. It is hard to vary policy in favour of one group without injuring another group. The task of the kennel master is formidable when every dog lives, in whole or in part, on dog.

State direction as originally conceived carried with it the implication of local

agents—in this case the C.A.E.C's. I note, in passing, their varying popularity as the great currents of public opinion ebb and flow—though it is but fair to the fickle public to admit that circumstances have changed. Their war work was a thing apart. They seized upon obvious weaknesses in farming and remedied them where they could, sometimes applying scientific tests, more often relying solely on human judgment. They greatly enlarged our horizon on cultivation problems. Intractable clays, for instance, which had broken the spirit of four horses, yielded before their giant implements hitched to the power of forty. For the purposes of my present theme, however, it is more important to note how their existence has facilitated the process of swaying agricultural practice. It is still true, as Lord Enle said, that ' You cannot move a great industry like farming with the same ease as a man turns over in bed'. But it is now possible, when any advance in agricultural science has reached the stage at which it can with safety be widely adopted, to bring to bear an enormous concentration of advisory power. The consolidation of the advisory service and their linkage with the Committees has had the same effect.

If we review progress in a few fields, taken almost at random from the lengthy list of enterprises which make up present-day farming, we find that the various forces or factors involved have played very varying parts. I take first the simple case of sugar beet. The sugar beet industry is, of course, artificial. It was created by the State for specific ends, and by common consent it has fulfilled the purposes for which it was designed. It naturally appealed in the first place to good arable farmers on the look out for a crop offering high returns. They started on the job without any tradition. Scientific men had little more information at their disposal than growers, but their means of acquiring it were greater, and evidence from the Continent was quickly applied, with modifications, to our conditions. Experiment proceeded at once ; the basic principles of crop nutrition were quickly worked out and demonstrated. The Corporation itself undertook technical investigations, appointing qualified fieldsmen. Help came also from implement makers, who were quick to adapt existing machines for use on the new crop—one of the earliest was so good that it remains in use, but little modified, today. New implements followed later.

One way and another, therefore, a great deal of advisory work, experiment and invention has been concentrated on the new industry. From the start the standard of cultivation, usage of fertilisers and crop yields have all been high.

The case of grassland is much more involved, though again the chain of research—experiment—exploitation—farm practice seems to have worked with smoothness and precision. In 1900 it seemed that the essentials of management were all to be found in the results of the Rothamsted Park hay plots ; certain fertilisers gave certain hay yields and, there was an end of the matter. Nitrogen was much the most important element ; some of the manures, if long continued, resulted in a changed flora, but these changes could be measured by analysing the hay botanically and, if necessary, chemically. The species were analysed individually but the analyses were very similar, and although we concluded that there were good grasses and bad grasses there was not much in the analyses to show which was which. In Northumberland, Somerville tried to measure manurial effects by grazing plots with sheep and recording their live-weight increase. His results were rather different. It appeared that slag and not nitrogen was the chief requirement, though it was not clear why. All the early reports from Cockle Park refer to the appearance of the grasses ; the role of wild white clover was not recognised until the results of later work by Middleton and Gilchrist were published. The idea of competition and co-operation amongst the components of the pasture flora began to grow : but even then the fundamental distinction between a pasture and a hay field was not noticed. The notion that the sheep had anything to do with the matter does not seem to have occurred to anyone—they were simply spring balances. The reputation of Wild White was, however, established—John Porter published a little book, *A great stride forward in Agriculture*, to advance its fame ; it figures prominently in Somerville's massive review in his presidential

address to this Section at Bournemouth in 1919.

Light came next from Hohenheim, where a strange doctrine had been evolved during World War I. Though 1 cwt. per acre of sulphate of ammonia was known to be bad for pastures because it suppressed clover, 5 cwt. was beneficial if the cattle grazing the pastures were appropriately managed. Rotational grazing made feasible the use of great quantities of nitrogenous fertiliser. At this point industry enters the ring in force.

Manufacturers of nitrogenous fertilisers conducted and promoted the conduct of a number of trials of rotational grazing, and broadly Hohenheim's results were reproduced in England, though some snags were discovered. It appeared that though the clover might endure the severe treatment the system involved, some of the grasses could not.

Woodman put the whole matter in perspective by linking output of nutrients with stages in defoliation. Simultaneously, Stapledon gave us a new view of the meaning of grass species, showing the existence of strains within the complex; the stem-leaf ratios varied in different strains. Fagan's analyses brought out strongly the nutrient value of leaf. We now began to think about plant physiology in earnest, and to realise that the grazed pasture was a two-way problem—the grassland influenced the beast, and the beast influenced the pasture. Industry threw in a novel device in the shape of an electric fence.

A perfect concert now began as research institutes, colleges, farm institutes, trade firms and pioneer farmers worked out the implications of strains and species, interactions, periods of rest, effects of depasturing, longevity of species, single-purpose and multi-purpose leys. There were some discords. The grass men wanted cattle put on and off to suit their beloved grass, while the cattle men enquired sardonically, 'What's the grass for anyway?' Muted for a time during the war, the concert rose to a crescendo afterwards as leys replaced cereals, and prices stimulated high production. Grass has been raised to the first place amongst crops as a producer of animal fodder. It is true we don't yet know how to measure its productivity exactly, various apparently sound means giving varying results. But there can't be any doubt about its potential.

Rather oddly, as it seems to me, one of the fundamentals of the business—the superiority of the ley over permanent grass—has never been subjected to satisfactory experimental test, nor is it easy to see how one can ever be designed; for temporary grass implies a system of management different from that of permanent grass. The question of whether the two types of sward differ *per se* in potential becomes somewhat academic.

In this particular progress the State has played mainly an indirect part, though assistance to grass driers and the organised campaign for silage may perhaps be classed as direct. Every link in the chain has thus come into play.

And what of the great mass of farmers? How widely are these new techniques employed? To a greater or lesser extent they are now firmly embedded in general practice. They are, however, adopted less widely in rearing and feeding areas than in the dairying regions. There are dairying areas where the average of fertiliser usage must be three or four times what it was, where farmer after farmer is intensively managing some fields, and a fair proportion adopt a system not far short of the experimentalist's model.

I hope I am not exaggerating if I suggest that the improvement in the purity of our milk supplies during the past half-century is worthy to rank with technical progress in grass management. The course of events and the rôles of the respective actors therein have, however, been quite different. The demand for a cleaner milk supply arose from outside the farming industry. It came from the public health authorities and—as a purely commercial matter—from distributors anxious to reduce losses in souring. The task which faced research institutes in respect of souring was really a very curious one. In a sense there was nothing to discover. All that was required was to keep your cows, your vessels and yourself clean; and in England, theoretically, every farmer was already practising these virtuous habits. It was an invidious task to tell him that his habits were dirty.

Stenhouse Williams's first job at the N.I.R.D. was to find some scientific

standard of dirtiness. He chose the number of bacterial colonies which would develop on a given medium in a given time. It was not really a very good measuring-rod, but it served its turn admirably. It became a sort of abracadabra to the clean milk campaign, a mystical symbol behind which all the advisory forces in the country—particularly the county councils—could march inoffensively but effectively ; and a vast leaflet literature was built up, elaborate competitions were conducted, a new attitude of mind was cultivated. There was no longer any need to tell a producer his milk was dirty ; one could say it had a count of 750,000. Whereupon he—or more usually she—went to look for them in unscoured vessels. The homely slighted vessel cleaning business was made to look like a scientific operation.

T.B. in milk was a tougher problem, for in the first place the method of test was imperfect and uncertain ; in the second place, the process of eliminating it from a herd was not one which lent itself to experiment. As in the case of the steps needed to produce milk with a low count, procedure could only be deduced from first principles, then applied by trial and error.

The State, hitherto content with Acts of prohibition—largely ineffective—now weighed in with a permissive Act establishing grades of milk. Though the milk Designations Order offered no rewards save a label, it gave an opening to far-sighted pioneers who had of course joined in the clean milk competitions and who now embarked on the venture of creating a market. We have come in the end to subsidy of course, but that does not detract from the merits of the original permissive Act. The movement has been greatly aided by improvements in materials—e.g. tinned steel, aluminium alloys, stainless steel—and latterly by a rather bewildering variety of labour-saving detergents and oxidising agents which have reduced the need for that primal but troublesome steriliser, steam.

A development rather out of line with the general stream has been the rise of modern poultry-keeping. Half a century ago poultry on the average farm were represented by a few barnyard fowl which the missus looked after, taking the eggs as pin money. We may judge the technical position by reference to the curricula in the agricultural colleges. Either there was no poultry-keeping at all, or if a place for the subject was found in the time-table, instruction was given by a lecturer in dairying. The chief locus of interest and of information lay in the poultry and pigeon societies, the most flourishing of these being in the Midlands, Yorkshire and, above all, Lancashire. Here a mere hobby, a partial insurance against unemployment, grew in the hands of a few zealots to a form of livelihood. Possibly the nearness of the ports gave them some advantage in obtaining foodstuffs, and doubtless the suppliers of these were not behindhand in selling their wares. Enough interest was eventually aroused to justify egg-laying competitions. These were frankly commercial in origin, but their publicity value was considerable, a fact which breeders were not slow to grasp. National societies, conferences, poultry parliaments followed. The poultry press grew ; journals serving little more than local hobby interests took on the stature of national organs. Something in the nature of a poultry industry—a wheel within the larger wheel of agriculture—had grown up.

Up to this point no steps had been taken to found research or experimental stations for poultry. The demand came from the industry itself, and it is significant of the sturdy faith of the leaders (for by this time forceful leaders had arisen) that they asked, among other things, for experiment stations where they could conduct their own trials on breeding. It was nearly two decades before research and experiment in poultry husbandry were organised on a basis comparable with that in dairy or fruit farming.

The industry has since learned much from the research worker both in nutrition and genetics. It has borrowed much from the United States (and at times has fallen into the same pits as poultry-keepers in that country); foodstuffs firms have conducted valuable trials ; endless ingenuity has been exercised in the design of equipment and housing. Opinions may well differ as to the identity of leaders of thought in this branch of farming ; for my own

part I raise my hat to that very homely figure the Lancashire working man.

I cite an exception (there are, of course, others) mainly to preserve a sense of perspective, and now return to the main theme of the educational team. We see the whole army deployed when we turn to animal husbandry. As I said earlier, this field was neglected in the early days. On the farm, practice was governed by an iron discipline. Of course, the rules for animal management were safe and sound; they could hardly be other, since they were proved by 2,000 years of experience, and they were kept with all the rigour of a guild. The only way of learning them was by apprenticeship. The shepherd strode with springy step and distant gaze, like stout Cortez, ' silent upon a peak in Darien'. If the cowman I knew best was a fair sample he imparted the mysteries of this trade mainly by a series of grunts ; he could hardly be called vocal. Science had but little specialised information, and that related mainly to the composition of foodstuffs. Even this, I am afraid, was interpreted mechanistically : to us the cow was a machine. The practical man was in this respect wiser than we. To him she was, as she always had been, an individual. Small wonder that he was suspicious of our advice. ' I wouldna trust un wi my osses ' comes back to me as a memory of a *sotto voce* remark overheard after an evening lecture on horse bots by a vet in the village school.

The breakdown of the barrier between practice and science came slowly. I think the first breach was made by Boutflour. It was a spectacular achievement. It required great courage in the 1920's to raise a cow to 7 gallons, prostrate her, and then, as she recovered, to raise her again to that dizzy height. Few people would have dared this ; especially with somebody else's cow. Sweeping away all vestiges of secrecy and sacredness came Marshall and Hammond's fundamental work on physiology and the processes of reproduction. Nowadays no one questions the ability of a scientific man to manage animals. What has really happened is of course that the ' laws ' of animal husbandry have been subjected to scientific analysis and found to be far more flexible than has hitherto been supposed. There still remains to define the state of bodily welfare known as ' condition', still measurable only by the master's eye—in all probability a summation of physiological functioning.

The animal is not an easy subject for the experimentalist, tied, as our earlier workers were, to the idea of balanced trials testing one variable at a time. Groups of considerable size maintained under uniform conditions were needed ; periods of trial were therefore short, and even then genetic variability resulted in high standard deviations. Devising of methods of individual feeding took some time. Short-period feeding trials could not, however, get us very far in evolving a working theory of management covering the whole life of the animal. The conception of a plane of nutrition was an advance. It is a vague definition, a half-way house between the exact standards of the laboratory and the crude criteria of the farm. Long-continued studies are, however, apt to become very involved owing to the changes and chances that beset animal life, and cause and effect are so interlocked that it is often difficult to decide which is which. An experiment to test the lifetime effect of methods of rearing dairy cattle, for instance, is complicated by dates of calving, incidence of worm infection, twinning, oestrus, and a host of other factors which may be casual or causal. If one divides groups to even out the effects of each variable, the groups are soon reduced to single animals ; on the other hand, the mathematical device of adjustments is not very satisfactory. Studies of a major problem often involve, therefore, a whole crop of component problems.

Where comparisons of breeding methods are concerned, large numbers and long periods of time are needed. Even with poultry, using stock which has been sibmated for anything up to twenty generations it has taken fifteen years and the whole resources of a considerable plant to establish effects of cross mating.

As one passes on to the ultimate test of economic results the difficulties multiply ; tests with cattle and sheep must, among other things, attempt to measure pasturage consumption, interaction between different types of stock, and effects of stock on sward ; and somehow or other pasture production must be converted into terms

of meat or milk returns per acre. What is the precise relationship between stock numbers and aggregate meat production per acre? How do results compare if sheep or cattle or both are used? And what is to be done about replication of plots? Cattle must have room to turn around in, but replication of large plots may defeat its own purpose by increasing land variability, even if the resources of one farm are great enough to provide the requisite area of similar crop.

Management trials represent the ultimate synthesis of work in many branches of science and thus call for a varied team—the soil chemist, the nutritionist, the agronomist, the veterinary surgeon, the economist, the animal husbandry specialist, all under the guidance of the statistician, all making their demands on the general manager. Experimental work can hardly grow more complex. Small wonder that it has had to wait till State experimental farms have come into being.

The pioneer farmer can hardly be expected to study whole life histories of animals as the experimental farms do, but these studies can be sectionalised; it is possible to conduct co-ordinated studies on practices affecting a portion of the lives of animals on educational and private farms alike, and by collation of records to obtain at least a rough picture of animal responses.

I suggested a few moments ago that the laws of animal management had proved to be more flexible than our forefathers thought. This fact is now widely recognised in practice. Bespectacled hens in dense throngs on deep litter confound tradition (and incidentally the hygiene of a decade or two ago). Pre-parturition milking and rearing of calves on two or three gallons of milk attest the fact that stock farmers—at all events some stock farmers—are as open-minded as their counterparts on the poultry farms. They are, therefore, fully prepared for any lessons management trials can teach. Some of the lessons are so obvious that they can be absorbed into practice without further trial—varying nutritional planes in pig feeding, for instance.

The main difficulty in applying them lies in their number and range, and the fact that on the farm they must all pass through the hands and head of one man. It is the same tale in whatever department of the farm one looks. All breeding plans, whatever the type of stock, call for far-sighted planning and production records laboriously accumulated and patiently applied. It becomes clearer and clearer as research and experiment proceed that maintenance of the health of stock is primarily a question of evading infection by skilful planning of feeding grounds and meticulous attention to nutritional needs. Moreover, the greater the intensity of the farming, the greater the need for skilful management, the heavier the burden that falls to the manager. Every advance in technology adds another straw to the camel's back.

Take, for instance, intensive grass management on a dairy farm. A single field, intensively managed, involves far more trouble than a field of corn. A whole farm of intensively managed grass calls for masterly planning, the establishment and maintenance of a series of leys, a programme of work which must be perpetually adjusted to fit changing and unforeseeable growth rates, alternative plans for hay, silage, and so forth. It may bring in its train a whole series of labour problems. And the grass is after all merely a means to an end. The manager's real task is milk production; it is the herd which demands his chief attention. No doubt on large farms the manager's load can be lightened by delegation. But the bulk of our farms are small; there is not much room for delegation on 100 acres.

Are we reaching the point at which the limiting factor to progress is human strength rather than technical knowledge? To my mind the most encouraging and hopeful feature of present-day farming is the proportion of men—particularly young men—who are willing to accept the full burden involved in applying technical advances, to adventure boldly and free of inhibitions to the making of a brave new world. It may well be that this spirit of adventure is no new thing, that it existed as a smouldering fire through the long era when rewards for enterprise were poor and uncertain. Sufficient that it now exists so patently in our midst.